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FOR LOGISTICS.[1]

INTRODUCTORY NOTE.—M. POINCARÉ AND M. COUTURAT.

Soon after Mr. Bertrand Russell's *Principles of Mathematics* of 1903 was published, M. Louis Couturat gave an exceedingly interesting popular account of this and other works in the *Revue de Métaphysique et de Morale* for 1904 and 1905, which was afterwards published in book form in 1905[2] with an appendix on Kant's philosophy of mathematics read at the celebrations in 1904 of the centenary of the death of Kant. Then M. Henri Poincaré thought fit to publish, in the above *Revue,* a series of articles of which this is a list:

"Les mathématiques et la logique," *Revue,* Vol. XIII, 1905, pp. 815-835; Vol. XIV, 1906, pp. 17-34; and *ibid.,* pp. 294-314; "La logique de l'infini," *ibid.,* Vol. XVII, 1909, pp. 461-482.

In connection with some of the subjects so lightly and gracefully touched upon by M. Poincaré appeared the following:

Mario Pieri, "Sur la compatibilité des axiomes de l'arithmétique," *Rev. de Métaphys.,* Vol. XIV, 1906, pp. 196-207.

Louis Couturat, "Pour la Logistique (réponse à M. Poincaré)," *ibid.,* pp. 208-250.

B. Russell, "Les paradoxes de la logique," *ibid.,* pp. 627-650.

The writings of M. Poincaré are well known to the readers of *The Monist.* His criticisms are refreshingly light and gay and he never allows profundity to obscure his wit. It is, however, unfortunate that his airy remarks on modern logic—which, by the way, he confesses rather needlessly that he has not studied—have been taken so seriously by many. It is, as newspaper editors know, a tendency of the public to read with interest and even to accept uncritically the opinions of an eminent person on matters about which he is not an expert. The views of a well-known football player on

[1] Translated by Philip E. B. Jourdain.

[2] *Les principes des mathématiques avec un appendice sur la philosophie des mathématiques de Kant.*

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the science of anatomy would no doubt be widely read, and the views of M. Poincaré on the philosophical questions at the root of mathematics are not, in essentials, of a very different nature. It is part of the business of anatomy to study deeply these faculties which the athlete uses unconsciously. The analogy is quite evident. M. Poincaré is one of our greatest mathematicians, and centuries have proved that a man who is a great mathematician need be neither a great philosopher nor a great logician. We do not expect such a combination of qualities,....nor, as a rule, do we find them.

M. Louis Couturat gave a very full and adequate reply to the first two of M. Poincaré's articles. In spite of this M. Poincaré reproduced, in the same words, his refuted arguments in his lately published book *Science et Méthode*. The chapter entitled "Les Mathématiques et la Logique" on pp. 152-171 of the book is almost identical with pp. 815-824 of the first article; the chapter entitled "Les Logiques nouvelles" on pp. 172-196, which is that translated on pp. 243-256 of *The Monist* for April, 1912, is an abridged version of pp. 826-835 of the first paper and the second article. The chapter entitled "Les derniers efforts des Logisticiens" on pp. 192-214 reproduces much of the less technical parts of his third article, and this article, which is translated in the present number, was replied to by Mr. Russell in his above cited paper.

The fourth article of M. Poincaré is concerned principally with a memoir on the theory of "logical types" published by Russell in 1908 and with one on the foundations of the theory of aggregates published by Zermelo in the same year.

It is quite obvious that nobody should allow himself to speak or write in terms of approval or disparagement of a branch of study with which he has only a superficial acquaintance. Each of us is free to dislike or like a particular subject and to leave it alone or to cultivate it accordingly, and if he finds good reasons for so doing he ought to publish them. But not even the most eminent can really think that a brightly written condemnation of a subject, based on a very superficial acquaintance with it, is of any real value. Indeed, the more eminent a person is, the more able he generally is to prevent us from seeing the truth. And then, besides the thought of the efforts of others to perceive the truth, there is the very noble sentiment with which M. Poincaré begins his book *La valeur de la science*: "The search for truth ought to be the aim of our activity; it is the only end which is worthy of it." Very nice, but with regard to what the French call "logistics" or "mathematical logic,"

and everybody used to call "symbolic logic," M. Poincaré has not been as true to his lofty sentiment as his admirers have learned to expect and demand.

Under these circumstances it seems only fair—I do not mean to logistics but to the public—to give people the opportunity to read M. Couturat's answer as well as M. Poincaré's attack.

In the following translation, any bibliographical or other notes which I have added are enclosed in square brackets. Where possible I have abbreviated the translation and avoided the use of symbols.

There are a few passages in M. Couturat's article which may possibly give rise to a wrong impression. Thus, he speaks of logical demonstrations making true the chance finds of the intellect. Of course the process of demonstration does not do this: It gives the finder and other people certainty as to whether the find is true or not. But we must not accuse M. Couturat of being a pragmatist on the slender grounds of a loosely expressed sentence; especially as in other parts of this article he has protested in the clearest possible way against the confusion between creation and discovery.

Near the end of the second section there is a reference to a number of mathematicians who failed adequately to deal with the paradox discovered by Burali-Forti, among whom are mentioned Russell and myself. The article of Russell referred to contains, implicitly, a criticism of certain views widely held by mathematicians at that time and also—again implicitly—the solution of the paradox and others like it. This of course was familiar to M. Couturat, but the citation of Russell in that connection might mislead some people. With regard to myself, at the time (1903-1904) that I wrote the papers referred to I was hardly, as M. Couturat says, "totally a stranger to logistics," but I freely grant that I was not as familiar with it as is necessary even to grasp the full bearings of the question. My attempt at the solution, though I believe it has one small merit in distinguishing between what may be called *entity* and *existence,* I have since then abandoned.

The discussion, in the third of M. Couturat's sections, of the question of *existence* does not appear to me to be satisfactory, and I have added a note referring to some former remarks of mine on this subject in *The Monist* for January, 1910. P. E. B. J.

REPLY TO M. POINCARÉ.

I thank M. Poincaré for the honor which he has done me by taking me in particular as the subject of his articles

on "Mathematics and Logic,"[3] but I must say that I do not deserve this honor, for the ideas which I have presented are not my own and I fear that M. Poincaré has done them a great wrong by discussing them from a work in which they are given at second-hand. In fact, as I have been careful to warn my readers, my articles[4] were mainly only an account of Mr. Russell's book; and wherever I have been led to add an analysis of the works of other logisticians I have not omitted to refer to them. Now it is not customary to criticize works of this class from a simple analysis of them, above all when the value of these works consists in the rigor of demonstrations, and these demonstrations are necessarily absent from my summary exposition. For example, I have analyzed long memoirs of Peano, Pieri and Whitehead by limiting myself to the enunciation of their chief theorems, without the quotation of a single demonstration. It is constantly assumed that the reader who wishes to see the demonstration of such and such a theorem has only to seek out the original memoirs, and it would obviously be pointless to reproach me for not having given it. Similarly I have thought I ought to describe in my book, to complete one of my articles, Peano's space-filling curve in an elementary and intuitive form which is accessible to the first comer and without speaking of the rigid analytical demonstration. What would one think of a mathematician who, only knowing this curve by my account of it, allowed himself to criticize its construction, to doubt the rigor of the demonstration, or to declare that this demonstration does not exist and that the proposition in question rests on intuition?

Also I had warned my readers that in my work I would

[3] [See Dr. G. B. Halsted's translation of "The New Logics" in *The Monist* of April 1912, and of "The Latest Efforts of the Logisticians" in the present number.]

[4] Published, with some corrections and additions, in a volume bearing the title, *Les principes des mathématiques*, Paris, Alcan, 1905.

sacrifice rigor to clearness, not to that *logical* clearness which is inseparable from rigor and which can only be obtained by logistical symbolism, but to that clearness in the common acceptation of the term which is called *intuition* and which M. Poincaré esteems so highly. It must be granted that I am very badly rewarded for the concessions which I have made to intuition, since M. Poincaré profits by them to reproach me with a lack of rigor. In any case, I wished to do the work of a commentator and a popularizer and to compose for the use of the laity a kind of introduction to the works of which I gave a short account. That is to say, it was not for M. Poincaré that I wrote, and I did not pretend to teach him anything about these works. In all cases, a work of the kind I wrote may serve—I hope so at least—to *teach* the elements of the doctrines in question, but it cannot serve as a sufficient basis to *criticize* these doctrines; to be just and effective, the criticism ought to be on the original works from which I drew my inspiration. What would M. Poincaré say if some one took it upon himself to discuss Hilbert's principles of geometry[5] from the analysis—however exact and complete it may be—which he has given of it to the French public?

I might stop with these remarks, and perhaps I ought to do so; for if I have already compromised the doctrines in question by my attempt at popularization, I run the risk of compromising them still more by undertaking to defend them against an adversary like M. Poincaré. If I have resolved so to defend them, it is, on the one hand, because it has pleased M. Poincaré to substitute me for the masters of logistics, and, on the other hand, because these masters have believed that I would suffice for the task and have left to me the care of justifying them. I thank them for

[5] [*The Foundations of Geometry*, trans. by Townsend. Chicago, The Open Court Publishing Co.]

their confidence; but the reader ought to know that if there is any thing good and enduring in my work it is to those masters that I owe it, and that all that is feeble and defective comes from myself. If then I succeed in justifying logistics against the criticisms of M. Poincaré, so much the better; if not, it will be my fault and will prove nothing against logistics.

I.

In the first place, we must not confuse logistics with what M. Poincaré calls "the logic of M. Hilbert." M. Poincaré has not made this confusion, but many of his readers may do so when they see him associate these two doctrines in the same discussion and in a common condemnation. Now it must be clearly realized that Hilbert is a complete stranger to logistics and has never used any logical calculus in his researches. If then the criticisms that M. Poincaré makes against him are just, they have no bearing against logistics, but rather tend to prove the superiority of logistics over verbal reasoning and simple common sense.

It is important also to correct a historical error to which the following phrase of M. Poincaré may give rise: "What Hilbert has done for geometry others wish to do for arithmetic and analysis." We might believe from this passage that the logisticians attack the subject of arithmetic and analysis *after* the works of Hilbert on geometry, and in imitation of them. The *Grundlagen der Geometrie*[6] of Hilbert were published in 1899. Now, ten years before this (in 1889) Peano had published not only his *Arithmetices principia nova methodo exposita* but also *I Principii di Geometria logicamente esposti,* both of which were written in the symbolism which he had invented in the year before. In 1891 he published in the first volume of the *Rivista di Matematica* two articles on the concept of num-

[6] [English translation as noted above.]

ber which already contained the five fundamental axioms of arithmetic. In 1894 he published in the fourth volume of the same *Rivista* the memoir on the foundations of geometry which I have analyzed in *Les Principes des Mathématiques.*[7] Lastly, in 1899 Pieri published his logical reconstruction of projective geometry and of metrical geometry in the Memoirs of the Turin Academy. These dates are enough to prove that, if Hilbert has not *wished* to profit by the works of the logisticians, the logisticians *could* not have profited by his work and had no need of his example not only in arithmetic and analysis but even in geometry. Consequently M. Poincaré commits a historical error in attributing to the "works of M. Hilbert" the "triumph" of logistics in geometry. I content myself, on this point, with stating a fact: In 1900 Hilbert elaborated for arithmetic a complicated system of *eighteen* axioms,[8] when *eleven years before this* arithmetic had been built up on *five* axioms only, which Padoa in 1902 reduced to four. Finally, to render to each person the "chronological" justice which is due to him, I should record that Frege stated, in his *Grundlagen der Arithmetik* of 1884, the theory of the integer number which Russell has adopted in principle, and undertook to prove that the principles of arithmetic are purely logical—analytical in Kant's sense.

M. Poincaré writes: "This invention of M. Peano was called *pasigraphy*," and adds: "This name exactly defines its bearing." The first phrase contains an error of fact. *Never* did Peano call his logical symbolism by the name of "pasigraphy"; he always called it "mathematical logic".[9] If I call it "logistics," it is first, because of the equivocal-

[7] Chap. VI, pp. 159-180.

[8] "Ueber den Zahlbegriff," *Jahresber. der deutsch. Math.-Ver.*, Vol. VIII, 1900. [This essay was reprinted in an appendix to the 3d German edition of the *Grundlagen der Geometrie*, Leipsic and Berlin, 1909, pp. 256-262.]

[9] See all the editions of Peano's *Formulaire de Mathématiques*, and the *Notations de Logique mathématique* (Turin, 1894) which forms the introduction to the first edition.

ness of the expression "mathematical logic," and, secondly, not because "this new name implies the purpose of revolutionizing logic," but because this good old word, which Vieta gave to algebra, indicates, by its very etymology, the general art of reasoning and calculating. In this sense it was employed in the eighteenth century by Lambert to denote his own logical calculus.[10] It was Schröder who first called it "pasigraphy" in a communication made to the first congress of mathematicians at Zürich in 1898, and that probably with a depreciative intention.[11] Now this word is quite inexact, whatever M. Poincaré may say. People call any written universal language a "pasigraphy"; thus the international code of maritime signals[12] is a "pasigraphy." I myself formerly used this word when speaking of Peano's symbolism but I corrected it at once: "A system of *pasigraphy* or, better, of *ideography*";[13] this means that the symbols translate not words or phrases but ideas. I concluded the same article by the words: "We would restrict incorrectly the value of Peano's symbolism if we only regarded it as a kind of stenography. It is also and chiefly an instrument of logical analysis, of deduction and of verification"; and I recalled, *à propos* of this, the "universal characteristic" of Leibniz. It is, then, entirely to misinterpret the nature and bearing of logistics to regard it as a mere pasigraphy.

For the rest, M. Poincaré speaks of logistics in the way in which a *bel esprit* might speak of algebra or mathematics in general. For example, he says: "The essential elements of this language are certain algebraic signs which represent the different conjunctions *if, and, or,* and *then.* That these signs may be convenient is possible, but that

[10] "Versuch einer Zeichenkunst in der Vernunftlehre, *Logische und philosophische Abhandlungen,* edited by John Bernoulli, Berlin, 1782.

[11] Translated into English in *The Monist* for October, 1898.

[12] Cf. Couturat and Leau, *Histoire de la langue universelle* (Paris, 1903), preliminary chapter on "Les pasigraphies."

[13] *Bulletin des Sciences mathématiques,* Vol. XXV, 1901.

they are destined to revolutionize the whole philosophy is another question. It is difficult to admit that the word *if* acquires, when it is written ⊃, a virtue which it did not have when it was written *if*." In the first place we must not believe that logistical symbols are limited to the literal translation of some words.[14] The sign ⊃ translates *if* no more than *then,* it expresses the idea of implication; the same sign may translate *and* in certain cases and *or* in other cases. Inversely, the word *and* has not the same meaning in the three following cases: "Peter is rich and happy," "Peter and Paul are rich," "Peter and Paul are brothers"; and consequently it is not translated by the same logistical symbol. It is, then, quite unjust to consider "the new language" as a mere tracing of ordinary language and consequently as having no value and no utility of its own.'[15]

M. Poincaré believes that I attach "an exaggerated importance which would astonish M. Peano himself" to the use of symbols. I can reassure him on this point. M. Peano writes to me on this subject: "I have always affirmed the importance of symbolic notation in all mathematical propositions, its great utility in difficult and delicate questions, and its indispensability in the study of principles. That is written down in all the volumes of the *Formulaire* ...."Everywhere and always he insists upon the necessity of expressing every mathematical proposition and every definition *entirely in symbols.*[16]

[14] Like the childish notations of Hérigone, who wrote, for example, "5<" for *pentagon*; or like any system of analogous abbreviations that a mathematical student may invent for taking notes.

[15] In 1895 Peano wrote: "Mathematical logic....does not reduce merely to an abbreviated symbolical writing, to a kind of tachygraphy; it allows us to study the laws of these signs and the transformations of propositions.... The two objects of mathematical logic, the formation of a symbolical script and the study of the forms of transformations (or of reasoning) are closely connected" ("Sur la définition de la limite d'une fonction," *American Journal of Mathematics,* Vol. XVII). This memoir was meant (as its subtitle "Exercice de logique mathématique" shows) to make the new logic known to mathematicians. Mathematicians then cannot be excused for still ignoring it, and it is doubly inexcusable for them to criticize it without knowing it.

[16] Cf. his memoir printed at Paris in 1900 among those read at the first international congress of philosophy.

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However that may be, there was some one who had an opinion which is as "exaggerated" as that of Peano and myself of the importance of symbolism, and that was Leibniz. He went as far as to say that the discoveries in mathematics that he had made arose solely from the fact that he had perfected the use of symbols, and his discovery of the infinitesimal calculus was, for him, only a specimen of his *characteristica universalis.*[17] In fact, we know that he did not invent infinitesimal *ideas*; he only invented a symbolism to represent them and an algorithm to manipulate them. We might say of him: "He only introduced two new signs, d and $\int$. That these signs may be convenient is possible; that they could revolutionize the whole of mathematics is incredible." We might also say of algebra: "It consists simply in representing by signs the words *plus, minus, multiplied by,* and *divided by.* But it is not to be seen how it constitutes a progress beyond arithmetic; it is difficult to admit that the word *plus* when it is written + acquires a virtue that it did not possess when written *plus.*" And yet, could the theory of equations and the theory of algebraic forms have been elaborated with *words*?

M. Poincaré asserts that "pasigraphy does not preserve us from error." Without doubt it does not, any more than the rules of algebra or arithmetic do. Does it follow that these rules are false or that we ought to defy them? Because we make mistakes in addition, must we condemn the four rules of arithmetic and even the arithmetical signs, and only count on our fingers or with little balls? The mistakes which a logistician may commit do not weaken the value of logistics any more than mistakes in calculation shatter the certainty of arithmetic. It is enough that logistics allows us to reason more easily and more surely and to discover faults of reasoning more easily; and that

[17] See Couturat, *La Logique de Leibniz,* pp. 84-85, the texts cited in the note and the third appendix.

is what it does. In this sense it is, as Leibniz said, an art of infallibility—not that logisticians are infallible, but they are less exposed to error than those who trust to simple common sense, that is to say to intuition.

Besides, M. Poincaré forms quite a false idea of logistics by considering it as a mechanism from which intelligence is nearly excluded; and his comparison of it with the "logical piano" of Stanley Jevons is not exact. We must first of all know that this logical piano merely concerns logical classes and that it only effects the least important —and the most mechanical—part of reasoning. Its office consists in suppressing the elementary classes which are annulled in virtue of the given premises. But it leaves almost all the rest to be done; thus, on the one hand, we have to put the logical problem into equations, and, on the other hand, we have to combine the subsisting classes in such a way as to obtain the consequences in the desired form. Thus the algebra of logic does not reduce to a blind mechanism. This is still more true of logistics which surpasses the algebra of logic and is much less "mechanical."

Another comparison is no happier: "Are the rules of perfect logic the whole of mathematics? We might just as well say that the whole art of the player of chess reduces to the rules for moving the pieces." But nobody ever asserted that all mathematics reduces materially to logic, that is to say that there is *nothing more* in a treatise on mathematics than in a treatise on logic. We maintain only that all mathematical reasonings are effected in virtue of the rules of logic *alone,* in the same way that all the games of chess that have been and can be played are effected according to the rules of the game...., otherwise the rules would be worthless. The comparison returns then against the adversaries of logistics, for it shows how a small number of elements, combined according to some few fixed laws, can generate an unlimited variety of consequences. People

have asserted that logistics put leading-strings on invention, and have urged against logistics the rights of genius. How could mathematics constantly evolve and progress if it is always condemned to rest on a small number of principles and "logical constants"? M. Poincaré does not use this argument and leaves on one side the question of invention; but it is clearly visible that the theory of "logical constants" inspires in him an instinctive repugnance, and that every attempt to catalogue the primitive notions and principles of mathematics appears to him to be an insupportable pretension and a restriction on the "liberty" of the scientific man. It is for that reason that he opposes to logical and demonstrative reason the "sure instinct" of the inventor and the "more profound geometry" which guides him; and these kinds of considerations are very much in fashion. It is, at the present time, fashionable to put the "logic of nature and of life" in opposition to formal logic that is disdainfully called "dialectical," "abstract," and "verbal."

There is here a confusion which it is important to dissipate. To oppose to logic the psychological fact of invention is to commit the most gross *ignoratio elenchi*. Logic has neither to inspire invention nor to explain it; it contents itself with controlling it and *verifying* it in the proper sense of the word (making it true). Do we reproach metrical science for not giving poetical genius or the science of harmony for not conferring musical genius? And do we therefore conclude that the rules of both have no value and no utility? As for the theory of "logical constants," the liberty of the mathematical discoverer is no more restricted by formulating the primitive principles and notions on which his science rests than the libetry of the musician, of the painter and of the poet is restricted by saying to them in turn: "As for you, you will never be able to do anything but combine the seven fundamental notes with

their accidentals; as for you, the seven colors of the spectrum, and as for you, the twenty-six letters of the alphabet." That is exactly in what measure logistics clogs invention and clips the wings of genius. People should really stop throwing invention at the head of logicians, as if invention could be contrary to logic. Besides, this "sure instinct" and this "more profound geometry" which guide the discoverer are only unconscious forms of the logical reason and proceed according to the same laws. The reason which invents is conformable, and at bottom identical, with the reason which demonstrates, and without it the latter could not *verify* what the former has by chance found; and these chance finds are only *true* on this condition. It is, then, conformity with the laws of logic "which alone gives value to the edifice which has been built."

M. Poincaré speaks of "the logic of Russell" and opposes it to the logic of Aristotle, as if Mr. Russell was the first to go beyond the confines of the Aristotelian logic. He appears besides to have an inexact notion of the Aristotelian logic when he says: "The logic of propositions of Russell is the study of the laws according to which the conjunctions *if, and, or* and the negation *not* are combined. It is a considerable extension of the ancient logic." I can assure M. Poincaré that Aristotle was already acquainted with the conjunctions *if, and, or* and negation, and that he took account of them in his logic. All the classical logicians knew and studied hypothetical judgments (where *if* figures), copulative judgments (where *and* figures), and disjunctive judgments (where *or* figures); and classical logic has always admitted *negative* judgments. If M. Poincaré means that Mr. Russell is the first who has translated these judgments into symbols and submitted them to an algorithm he is at least half a century out of his reckoning: for it is to Boole (without speaking of his fore-runners)

that this honor is due. It is, then, not Mr. Russell who has "adjoined" to syllogistics "the conjunctions *and* and *or*" and who has thus "opened up a new domain to logic."

M. Poincaré believes that he can establish a fundamental difference between the two logics by remarking that "the symbols are multiplied and permit of varied combinations *which are no longer finite in number*," and he adds: "Have we any right to give this extension to the meaning of the word *logic*?" It would, then, seem that for him logic is characterized by the *limited number* of the combinations which it admits. But I do not see that there is a radical difference. Besides, in what sense did the ancient logic only admit a limited number of combinations? Is it a question of the number of valid moods of the syllogism? But modern logic, too, only admits a limited number of simple types of reasoning. Is it a question, on the other hand, of the infinite diversity of complex reasonings that one can obtain by combining these types? But classical logic too could form an infinity of reasonings by combining syllogisms. In all cases the two logics have the same character and only differ in respect of the more or less. Besides, how is the number relevant in this matter? If a logical principle is true, whether it be the principle of the syllogism or any other, is it not capable of justifying an infinite number of reasonings just as well as a finite number? Does its demonstrative virtue by some chance become exhausted after n applications? Lastly, what means this reproach addressed to logistics of admitting an infinite number of combinations, when, on the other hand, it is reproached with only having a very limited number of principles? Is it not, rather, for it, just as it is for geometry (according to a well-known phrase), a glory to deduce from so small a number of principles so great a number of consequences? How can this fact scandalize a mathemati-

cian who is familiar with the incredible fruitfulness of the theory of combinations?

When M. Poincaré opposes the old and the new logic to one another and considers the latter as an enormous and perhaps illegitimate "extension" of the former, he appears to forget the fact that the *domain* of a science may receive an extension—even a considerable one—without the *notion* and the definition of this science changing. Otherwise we could never speak of the progress of the sciences: M. Poincaré seems to suppose by that that a science remains in essentials identical with itself in the course of its historical development. The reasoning of M. Poincaré would serve to prove that the infinitesimal calculus is not a part of mathematics; that electricity is not relevant to physics, and that the theory of organic compounds is not relevant to chemistry. Now it is for this reason that the extension of the "field" of classical logic becomes an extension of the "meaning of the word *logic*." M. Poincaré says again: "It seems that there is nothing new to write about formal logic and that Aristotle saw to the bottom of it." If he means by that (as Kant did) that logic has made no progress since Aristotle, it is nowadays a simple error of fact; but if he means that logic ought to remain (or ought to have remained) confined in the domain assigned to it by Aristotle, he maintains implicitly that logic was perfect and complete at its birth, and this is contrary to the analogy of all the other sciences and to probability. We would only smile at a man who claimed to reduce mathematics to what it was in the time of Euclid, and physics to Aristotle's physics. How then dare any one maintain or insinuate that Aristotle has said the last word about logic and that it is forbidden to develop this science beyond the narrow limits assigned to it by its founder?

Besides, if "the new logic is richer than the classical logic," it is not so much by the extension of its domain as

by the deep study of the principles that have *always* directed those reasonings which have been recognized as just by that rational instinct to which M. Poincaré attaches so much value. He seems to reproach the logisticians with "introducing" into logic indefinable notions and indemonstrable principles. It would be more just to say that they have discovered or recognized them; just as Aristotle did not invent but discovered and recognized the principle of the syllogism. M. Poincaré is in too great a hurry to assert that these indemonstrable principles "are appeals to intuition, are synthetic *a priori* judgments." Perhaps he would have been of another opinion if he had taken the trouble to run through the enumeration of these principles. Why should the principle of composition: "If *a* is *b*, and *a* is *c*, then *a* is *bc*" constitute an appeal to intuition rather than the principle of the syllogism: "If *a* is *b*, and *b* is *c*, then *a* is *c*"? In what is the principle of simplification: "*ab* is *a*" more synthetic than the principle of identity with which it has been so often confused? In any case, it has been considered by Kant as the type of analytic judgments. Is it of these principles that M. Poincaré said: "We regarded them as intuitive when we met them, more or less explicitly enunciated, in treatises on mathematics. Have they changed character because the meaning of the word logic is enlarged and we now find them in a book called *Treatise on Logic*"? In what treatise of mathematics has M. Poincaré seen them formulated? And his argument returns on himself, for even if they were put in a treatise on mathematics, would that change their character as logical principles? "*They have not changed their nature, they have only changed place,*" writes M. Poincaré in italics; but it is he who has changed place. It is not enough that they should be used in mathematical reasonings to call them mathematical, and it is not enough that they are not found in treatises on classical logic to refuse to them the title of

logical principles. Otherwise it would be necessary to say that *logical* principles are, by definition, those which Aristotle and the schoolmen have discovered and formulated; and that all the logical principles discovered by modern logicians are *intuitive*. The distinction of the logical and the intuitive would then reduce to a question of chronology.

Besides, the vague conception of *intuition* is out of place as a weapon against the logicians, especially when the intuition spoken of is not specified. Is intellectual intuition meant, which bears upon the relations of ideas, or sensible intuition, which necessarily clothes the spatial form? These two intuitions are wholly different. All logicians are ready to recognize that their principles proceed from intellectual intuition, that is to say they are objects of immediate knowledge by the reason; but very few will agree that they proceed from sensible intuition, and rest, for example, as Lange has maintained, on spatial schemata. For the rest, whatever the solution of this "metalogical" problem may be, all the logical principles ought to have the same fate; and the traditional principles of identity, contradiction and so on will be "appeals to intuition" in the same sense and in the same measure as the others. The logisticians then, must not be accused of altering logic by introducing intuition into it; for if this accusation has any value, it is Aristotle who began this introduction.

In any case it is inexact to say that "living" reasonings, the only ones "in which our mind remains active," are "those in which intuition still plays a part." Purely logical reasonings need more mental effort and ingenuity than M. Poincaré believes, and, even with the mediocre aid of Jevon's logical piano, a certain cleverness is necessary to combine the brute results of mechanism and to draw the conclusion wished. Besides, why reproach logistics with

making reasonings easier and more sure?[18] If, like algebra, it condenses into short formulae the result of long and complicated reasonings, it is to relieve the powers of the mind and to allow it to embrace a greater number of data and to draw vaster and more distant conclusions. Consequently, far from paralyzing the faculty of invention or rendering it useless logistics lends it stilts or wings. The discovering mind will always find something to exercise itself upon, but it will do so on data which are more and more complex. That is what happens in analysis, where each new theory combines formulae which sum up the results of simpler and more elementary theories. M. Poincaré may then be reassured: logistics does not exclude genius.

M. Poincaré makes a curious reproach to logistics: "The part of intelligence is restricted to choosing among a limited arsenal rules posited beforehand, and has not the right to invent new ones." If we remark that the "rules" are none other than the principles of logistics, this phrase appears to me to mean that intelligence "has the right" to invent new logical principles. It is a strange conception of logic to consider it as always evolving and as never finished.[19] It evidently proceeds from the psychological confusion between the science and what we know of it at a given moment. No one will ever "invent" new rules of logic; some of these rules which had not been noticed but were quite as "ancient" as the others and equally "posited" beforehand, that is to say *a priori,* will perhaps be "discovered." And the logisticians do not do anything else. But then, why does M. Poincaré reproach

[18] "More sure," for M. Poincaré confesses that in living reasonings "it is difficult not to introduce an axiom or postulate which is unperceived." Must we conclude from that that "life" is incompatible with logic?

[19] To use the favorite comparison of M. Poincaré, what would we say of a chess-player who wished to invent a new rule in the middle of a game,—for example, to make his king move several squares when in check? Such an "invention" would be called trickery and nothing else.

them with innovating? With respect to the nine indefinable notions and the twenty indemonstrable propositions of Russell, he says: "I believe that....I would have found some more." He is quite at liberty to do so: the logisticians do not ask for anything better, and will register his discoveries —or, if he prefers to say, his inventions—with gratitude. But what do these contradictory reproaches mean if not that M. Poincaré claims for himself "the right" to "invent" logical principles at the very moment when he refuses this right to the logisticians?

For the rest, what good is it to discuss *in abstracto* the qualities of logistics? M. Poincaré grants that "pasigraphy can furnish us with a criterion to decide the question which occupies us. If every treatise on mathematics can be translated into the Peanian language, the logisticians are right." Now the logisticians replied in advance, long ago, to this ironical invitation. Ten years ago Peano published the first edition of his *Formulaire de Mathématiques*, which is precisely a treatise or manual entirely written in logistics; the fourth edition (1903-1904) comprises Logic, Arithmetic, Theory of Numbers, Algebra, the Theory of Real Numbers, the Theory of Definite Functions, the Infinitesimal Calculus, the Theory of Complex Numbers, the Theory of Circular Functions, the Geometrical Calculus (comprising the theory of vectors and the theory of Quaternions), and Differential Geometry; the "Additions" even contain the elements of kinematics. The fifth edition of the *Formulaire* is in course of publication.[20] The principal theorems are accompanied by their logistical demonstrations. I will add that this mathematical manual is a collective work which M. Peano and his collaborators are incessantly revising and perfecting. Consequently the proof

[20] Professor Peano has published, besides, a classical manual entitled: *Aritmetica generale e algebra elementare*, drawn up in logistics (Turin, 1902).

which M. Poincaré requires of logisticians was given long ago and is being completed from day to day.

It is true that M. Poincaré soon seems to repent of his rash concession and adds: "Again we must examine the translation closely. It is not sufficient that we should be presented with a single page where there are only formulae and not a single word of ordinary language, in order that we must bow down....It will be necessary, when we are in the presence of a pasigraphical reasoning, even when this reasoning is correct, to examine if an appeal to intuition is not hidden away in some corner." These reserves are evidently very just in so far as they are counsels of critical method. But why does M. Poincaré not conform to them? It is not enough to express these general reserves which are applicable to any demonstrative work, to weaken the value of logistics and throw disfavor and suspicion on the work of logisticians. The logisticians have given to the public not "one page" but more than three hundred pages of logistical formulae and demonstrations. Let those who have doubts on the value of these demonstrations "examine" them as closely as they wish and let them point out lacunae and errors,—for that is their right and even their duty. But the burden of proof falls on them, and it is not enough, in order to get rid of this burden, to shake their heads with a smile of incredulity.

II.

I pass on to the objections aimed at logistics in so far as it is applied to mathematics. Here again I must say that M. Poincaré wrongs it by judging it merely from the "popular" exposition which I have given of it. In effect, the logistical formulae which constitute, as M. Poincaré says, a "new language" are sufficient by themselves and are intelligible wholly by themselves; if it were necessary to add to them a single word of ordinary language, it would

prove their incompleteness or defectiveness. Besides, this "new language" was invented expressly to avoid the equivocation or the beggings of the question implied more or less confusedly in ordinary language. Consequently the logistical formulae are the *only* ones which can be exact, rigorous and exempt from the above logical vices. Thus, when an author thinks that he ought to translate them into ordinary language, it is merely to make them more accessible to the "laity"; but it must be understood that this verbal translation is always imperfect, approximate and by no means allows the proper appreciation of the logical value of the formulae. Just because language cannot equal the precision and the rigor of the formulae, I have made no scruples about introducing into my verbal translations apparent beggings of the question in order to make them more clear and more "French." What does an inexactitude more or less matter when the logistical formula alone counts from the logical point of view? I could not expect that any one should judge and condemn these formulae from the mere inspection of the verbal translation which I gave of them for the use of novices. All translation is a betrayal; but that is still more true when the translation makes exactly those qualities of the original on which study and discussion bear vanish. It is exactly as if some one wished to study the meter of Virgil in a French translation of the *Aeneid*.

Now, it is of these verbal translations, *and only of these verbal translations*, that M. Poincaré has taken account in his criticism; he does not appear to have noticed the logistical formulae; "It is Greek, so it is unread." He may then "amuse himself by counting how many numerical adjectives my exposition contains": that will prove absolutely nothing against "pasigraphy." Nevertheless we will examine his arguments one by one in order to show better that they all miss the point. On the subject of the logical

definition of zero, he says: "to define *zero* by something *null* and something *null* by *none* is indeed to misuse the richness of the French language." Then he recognizes that I have "introduced an improvement in *my* definition" (a double inexactitude, for this definition is not my own and the "improvement" in question is due to Mr. Russell) by writing "what," according to M. Poincaré "means, in French, zero is the number of objects which satisfies a condition which is never satisfied. But as *'never'* signifies *'in no case'* I do not see that the progress is very great." I will confine myself to recalling the verbal translation that I have given of this formula: "if φx is always false, Λ is the class of x's which verify φx." The verbal translation of that is: Λ is the class of objects which satisfy a condition which is *always false,* that is to say, false for *all* the values attributed to x. Where is to be seen in this formula the idea of the number zero or even of any number? And are we to be reproached for introducing into logic mathematical notions, when classical logic was acquainted with universal judgments and used the word *all*? To be able to attribute to us a begging of the question—even one that is simply verbal—M. Poincaré has had to transform our translation by replacing "always false" by "never true." If, then, somebody here abuses the French language it is not I.

But this reproach is even more undeserved if it is addressed to the logistician, who writes neither in French nor in Italian nor in English but in a symbolism made expressly to liberate ideas from the tacit implications that language introduces into them by custom. M. Poincaré himself says: "It is impossible to give a definition without enunciating a phrase and difficult to enunciate a phrase without putting in it a name of a number or at least the word *many* or a word in the plural.[21] And then the roof

[21] On the subject of the plural, it may be remarked that Peano has, follow-

is slippery and at every moment there is a risk of falling into a begging of the question." These very just reflections bear only on the logical defects of language and on the faults that language can make us commit. It is precisely to avoid these faults and to cure these defects that the logisticians have invented their rigorously defined signs which have no meaning but that which they are given by definition.[22] Put shortly, M. Poincaré's argument comes to this: "All those who reason with the words of ordinary language are *liable* to commit beggings of the question; now the logisticians use, not words, but symbols rigorously defined; consequently they too *must* commit beggings of the question." The syllogism is not conclusive for it has four terms. And even if it had only three, that is to say when one could legitimately conclude from words to symbols, the two words which I have italicized would still render it invalid; the major says that we *may* commit errors; the conclusion asserts that certain authors have *necessarily* committed them.

The criticism of the definition of the number 1 is no firmer. "*One* is the number of elements of a class of which any two elements are identical"; such is the verbal translation that M. Poincaré gives of this definition. "It is more satisfactory....in the sense that, in order to define 1, we do not use the word *one*;—but still the word *two* is used"; and M. Poincaré rightly suspects that *two* can only be defined by means of *one*.[23] But he makes an unjust use of the fact that I have used the word *two* to make a phrase in ordinary language. The more exact translation of the logistical formula is: "1 is the class of classes u which are

ing Leibniz's recommendation, excluded it from the "uninflected Latin" which he has given out as a form of international language, and which has been adopted by many.

[22] Cf. the beginning of the preface to Peano's *Arithmetices principia* of 1889.

[23] By means of the general formula by which we define $n+1$ by means of n; cf. *Les Principes des Mathématiques*, Chap. II, § B, p. 59.

not null and such that if x is a u and y is a u then x is identical with y whatever x and y may be." Where is there, I do not say the word, but the idea of two in this formula? M. Poincaré will say perhaps that *two* (problematic) elements x and y of the class u are made to appear in it; but the fact that they are *two* does not come into the question in any way; and the proof of it is that in reality they are only *one*: x and y are merely two names (excuse me, *names*) for a single individual. This criticism obviously has no bearing on another equivalent formula which I have given,[24] and which may be translated: "*One* is the class of classes u which are not null and such that if x is a u the class of the elements of u which are not identical with x is null." That presupposes of course the definition of the null class; but, as we see, there is no more even a problematical *two* elements of u, but only *one*, and we only express that there is no other.[25]

Will anybody say that, by the mere fact that *an* element is spoken of, the number *one* is implied?[26] But that is an objection which M. Poincaré does not formulate and to which I have replied in advance in the following passage: "We must not believe that the definition of the number *one* constitutes a vicious circle, for the definition of the singular class rests solely on the relation of identity. If it is true that it implies in a sense the *unity* or rather the *individuality* of the element considered, this unity cannot be identical with the *number one* which is to be defined: for this unity is a property of each element while the *number one* is the property of a class. . . . consequently, in all cases

[24] *Ibid.* I have logistically deduced this from the preceding one on p. 60.

[25] Here is a more fundamental definition that Mr. Russell has communicated to me: "*One* is the class of classes u such that the proposition: 'x is a u' is equivalent, for all values of x, to 'x is identical with c' is not false for all values of c.'" Notice that this definition does not presuppose the notion of the null class. As for the formula "x is a u", cf. its definition farther on.

[26] [In French, the same word *un* stands for both *an* and *one*].

the *units* which constitute a cardinal number are different from the number *one*."[27]

The confusion which exists in many minds between these two ideas arises, I believe, from the double meaning of the word for one, which is used both as the name of a number and as an indefinite article.[28] In the latter case it would be better to use the word *some* as the logicians do.[29] This equivocalness exists in French and German, but not in English. If then, somebody is inclined to invoke it, he should take care to abuse not "the richness" but the poverty of the French language. To sum up, it is not enough to conceive *any one* object to conceive the *number one,* nor to think of two objects together to have by that alone the idea of the *number two.* From the fact that a logical formula contains two or many symbols we must not conclude that it implies by that alone the idea of two or of some other number. When we say: "Peter and Paul are wise," we mean to say: "Peter is wise and Paul is wise"; we do not think the *number two* and we have no need to think it nor to notice that that makes "two wise men." In the same way when we say: "x and y are elements of the class u,"[30] we do not think the number two and no idea of number is implied in this proposition.[31]

[27] *Les Principes des Mathématiques,* Chap. II, § A, pp. 47-48. M. Poincaré seems to propose or to accept such a justification when, after having quoted the phrase of Hilbert: "Let us consider the object 1," he adds: "Remark that by doing this we by no means imply the notion of number, for it is understood that 1 here is only a symbol...." Doubtless, but it is *a* symbol, that is to say *one* object. Will M. Poincaré say that that implies the *number one?* Or will he grant to the logisticians the same liberty as to Hilbert?

[28] [Cf. note 26].

[29] And also M. Méray, thus giving example in logic to other mathematians.

[30] Notice that it is only grammar which makes us use the sign of plural in *are elements.*

[31] Here is the rigorous definition of the proposition "x is a u," that Mr. Russell has communicated to me: "x is a u" means: "The proposition: 'ϕx is true, and u has the relation of a class to the property which defines it' is not false for all values of x." There is not here the shadow of the idea of the number one, but, as in my enunciation, the purely logical notions of *false, negation* and *all.* This definition was already given by G. Frege, *Grundgesetze der Arithmetik,* Vol. I, 1893, p. 53.

These considerations reply to this objection of M. Poincaré's: "A relation is incomprehensible without two terms; it is impossible to have the intuition of the relation without having at the same time the intuition of its two terms." That proves nothing, and M. Poincaré adds: "And without remarking that they are two, for in order that the relation may be conceivable, it is necessary that they should be two and two only." It is not the question to know—and it is a psychological question—if we "remark" or not that they are two, but if the notion of the relation implies that of the number *two*. Now for that it would be necessary that it implied the notion of the class formed by its "two" terms; and that is obviously not the case. The proposition: "x is the father of y" by no means implies the idea of the class formed by x and y. Besides, it often happens that a relation (which is then called reflexive) exists between a term and itself. Would it be maintained then that it has still two terms? That would be to say that x is at the same time *one* and *two*.

The only logistical formula that M. Poincaré has criticized in itself and not in its verbal translation is one given by Burali-Forti. M. Poincaré says on this point: "I understand the Peanian language too little to dare to risk a criticism." This confession would disarm us if he did not "risk" this criticism immediately afterwards: "I fear that this definition begs the question, for I see the figure 1 in the first member and '*Un*' in the second member." M. Poincaré has trusted too much to his "intuition," and it has deceived him. Instead of "risking" this criticism on the mere witness of his eyes, he ought, conformably to the fundamental rule of mathematical method, to have substituted for what is defined the phrase which defines it; and to ascertain if this definition really begs the question, he had only to refer to the definition of the symbol *"Un."*

Now M. Burali-Forti defines *"Un"* as the class of sin-

gular classes, which in Russell's definition of the *cardinal number* 1. This definition is equivalent to the one which I have given above and neither of them implies the idea of that which is defined. As to the formula which M. Poincaré has criticised, it means: "1 is the ordinal type of the ordered classes of which the cardinal number is unity." Thus it consists in defining the *ordinal* number 1 by means of the *cardinal* number, and this explanation is enough to do away with any appearance of a vicious circle. So we see how "risky" the criticism of M. Poincaré is.

He seems to consider as insignificant the formula

$$1 \varepsilon \text{No}$$

which M. Burali-Forti deduces from his definition. M. Poincaré translates it inaccurately as: "One is a number"; and then makes merry at the expense of pasigraphy, which "is sufficient to demonstrate that one is a number." If he had read the memoir of M. Burali-Forti—even in the "interlinear Italian translation"—more attentively he would have known that "No" means ordinal number, and perhaps he would have found the formula which teaches us that 1 is an ordinal number less ridiculous. Even if this formula "taught" nothing to M. Poincaré, he had no grounds for judging it to be insignificant, and that for two reasons. On the one hand, this formula is sufficient to prove that the class "No" exists, and this result is not to be despised, since M. Poincaré attaches so much importance to existence-theorems and wrongly reproaches the logisticians with neglecting them. On the other hand to prove that all the finite whole numbers are ordinal numbers, we are obliged to use the principle of induction, and for that purpose to set out from the fact that 1 is such a number. However evident or trivial this fact may appear to M. Poincaré, it was important to demonstrate it, and the formula at which he mocks proves the conscientiousness and the rigor of the

logisticians. The pleasantries of M. Poincaré are then quite pointless.

As for the paradox discovered by M. Burali-Forti in the theory of transfinite ordinal numbers, and from which M. Poincaré deduces an argument against logistics, I will only say that this contradiction can by no means be imputed to the use of logical symbols; and the proof of this is that mathematicians who are total strangers to logistics recognize it, discuss it, and have for years past spent vain efforts to solve it.[32] It is a purely logical difficulty which resides in the principles of the logic of classes, that is to say in the old and traditional part of logic. M. Burali-Forti, in a communication made to me,[33] believes that the contradiction arises from the different meanings that are given to the word "ordinal number," and that it depends, at bottom, on the extension and the properties attributed to the concept of *class*. Mr. Russell believes that it can only be solved by restricting or even sacrificing the notion of *class*; broadly speaking, we must give up the principle—apparently so evident and clear to intuition—that each concept determines a class which is its extension.[34] If logistics has enabled us to discover this contradiction, it can only be considered as a merit and not as a reproach for it proves that it is an instrument of precision for thought. But M. Poincaré is more exacting. He summons logistics to re-

[32] Bernstein, *Math. Ann.*, Vol. LX; Jourdain, *Phil. Mag.*, 1904-1905; Russell, *Mind*, 1905; Hadamard, Borel, Lebesgue, Baire, *Bulletin de la Société Math. de France*, 1905; Zermelo, Borel, König, Schönflies, *Math. Ann.*, Vol. LIX, LX.

[33] [Cf. for fuller details pp. 228-229 of Couturat's original paper].

[34] See Russell, "On some difficulties in the theory of transfinite numbers and order types," *Proc. Lond. Math. Soc.* (2), Vol. IV, 1905, pp. 29-53. M. Poincaré concludes hastily: "Burali-Forti and Cantor have arrived at contradictory conclusions; thus one or the other is mistaken." It cannot be said that one of them is mistaken if it is a question, as Russell shows, of a contradiction of principle, of a kind of antinomy. Thus we can see how much the conclusion is worth: "consequently pasigraphy does not preserve us from error." For the rest, logistics is only a "method of infallibility" (as Leibniz said) if certain premises are granted; it cannot be made responsible for a contradiction inherent in the premises.

solve here and now the contradiction which has become the crux of mathematicians. He says of Mr. Russell and Dr. Whitehead: "If they could have....purged it [the theory of infinite numbers] of every contradiction, they would have rendered us a signal service." The logisticians are not obliged to solve difficulties which stop all mathematicians, M. Poincaré included, and—as if they were modern Oedipuses — to reply to the riddles of all the sphinxes which are encountered in science; but if they succeed where others have failed, M. Poincaré will be good enough to remember this phrase, and do honor to logistics for the solution.

III.

I now come to the special criticisms that M. Poincaré addresses to the logisticians on the subject of their philosophy of mathematics and, in particular, of their theory of whole number. In the first place, there are certain arguments which it is astonishing to find him using, but which fortunately are not likely to impress philosophers. For example: "The definitions of number are very numerous and very varied....If one of them were satisfactory, no new ones would be given." The same objection might be urged not only to every philosophical speculation—and that is the usual argument of sceptics and positivists—but to every scientific *theory*; M. Poincaré knows this quite well. If this argument had any value, it would be the negation of all progress, even scientific progress. In mathematics in particular there exist numerous definitions of the irrational number, of the limit, of the definite integral, and so on. Has ever any one concluded from this that all these definitions are bad? Certainly not, but simply that certain ones are better than the others, without these others being properly speaking defective or wrong. For the rest, if this argument were to be taken literally, it would prove at the

outside that all the definitions proposed are bad *except one,* the last. Consequently, the argument has no bearing against Mr Russell's definition as long as this definition is the last proposed.

M. Poincaré is surprised that the logisticians define arithmetical addition by means of logical addition which appears to him to rest on an act of intuition which is analogous but "more complex." But in the first place if an act of intuition is really necessary for one or the other of these operations, is it not advantageous and meritorious to define the one by the other, so as to reduce to a minimum the number of acts of intuition? Logical addition is not an invention of the logisticians; it has existed at all times and in all minds. It is the combination which the conjunction *and* expresses in the phrases, " the French and the English," "philosophers and mathematicians." Logic, even classical logic, cannot dispense with it. Thus it is not arbitrarily, as M. Poincaré seems to believe, that this notion is introduced "into the chapter headed 'Logic.' " Given that it is indispensable to logic, the whole question is to know if it can be used to define arithmetical addition. This idea is too natural for Peano and Russell to have been the first to do it; it is already clearly expressed in the work of Lambert. To refute it, M. Poincaré ought to have shown how and why arithmetical addition cannot be defined by means of logical addition, and consequently ought to have criticized Whitehead's[35] formal definition of it. Or, if he believes that the notion of logical addition is "more complex" than that of arithmetical addition, he should try to define the first by means of the second. That is the best means of proving that mathematics is independent of logic. Meanwhile, he ought to allow the logisticians to observe the classical precept that principles must not be multiplied without necessity.

[35] *Amer. Journ. of Math.,* Vol. XXIV, 1902.

M. Poincaré solemnly accuses the logisticians of having violated two rules of method. The first consists in this: Every mathematical definition supposes the existence of the object defined and is only valid on this condition. But this condition is by no means a necessary rule. It is useless to invoke the opinion of John Stuart Mill, whose authority is rather mediocre in the logic of mathematics. The condition that M. Poincaré wishes to impose on logisticians is absolutely gratuitous and is not acknowledged by the most rigorous mathematicians. A definition is no more than the giving of a name; it by no means supposes the existence of its object. We can very well define a problematical object, and then prove that it does not exist. Thus Euclid denotes by a certain sign "the greatest prime number," and then demonstrates that it does not exist. We define the derivative or the integral of a function in general, without supposing that every function has a derivative or an integral. What M. Poincaré wished to say or ought to have said is, on the other hand, that a definition does not *imply* the existence of the object defined, and this existence must be proved or postulated if we wish to be able to use it in further reasonings. This[36] is a well-known rule of mathematical method, and it is enough to run through Peano's *Formulaire* to see that each definition is accompanied, when there is occasion, by an existence-theorem which usually determines the conditions under which the object defined exists.

M. Poincaré says that "in mathematics the word *exists* can only have one meaning, it means '*is exempt from contradiction.*' " I am sorry to contradict him on so elementary and essential a point: logical—or mathematical, it is all one—existence is quite another thing from the absence of contradiction.[37] It consists in the fact that a class is not

[36] Cf. *Les Principes*, 39.

[37] It is a curious thing that this conception of logical existence only appears admissible in a *panlogism* analogous to that of Leibniz, and where the exten-

empty; that is to say that at least one member of it exists, and this means by definition that the class in question is not null. It is exactly for that reason that it is the custom of mathematicians to prove the existence of a class by giving an *example,* that is to say by indicating an individual which belongs to this class; and they have no other means for proving an existence-theorem—unless they reduce it to a preceding theorem or postulate of existence.[38]

But, it will be said, how is the existence of the individual which is used as an example proved? Must not this existence be established in order that the existence of the class of which it is a part may be deduced? Although this assertion may seem paradoxical, the existence of an individual as such is not demonstrated. The individuals, by the mere fact that they are individuals, are always considered as existing; or rather the question does not arise for them since logical existence is a property of classes and not of individuals.[39] We never have to express that an individual exists, absolutely speaking, but only that it exists in a class, that is to say, is an element of it.[40] When an individual is defined by means of general terms, its existence is demonstrated in two stages: This individual being defined as *the* u (u being a certain class), we demonstrate that the class u is not null, and then that it is a singular class. The defi-

sion of concepts would be absolutely determined by their comprehension. For example, Leibniz and his disciples believed that if "No man is a stone," that is to say, if no "men-stones" exist, it is because the concepts *man* and *stone* respectively contain contradictory elements (such as *living* and *not-living*).

[On the following discussion of the "existence" of classes and individuals, cf. my remarks in *The Monist,* Jan., 1910, Vol. XX., pp. 113-116.—Tr.]

[38] It is enough to have a proposition of the form: "x is a member of u," to be able to conclude that the class u exists.

[39] Of course, classes themselves can be considered as individuals with respect to classes of classes, but then they "exist" even when they are null.

[40] M. Poincaré thinks it necessary to add to Peano's postulate the following: "Every integer has one which follows it." He does not see that this postulate, which he believes new, is contained in the third postulate: "The consecutive of an integer is an integer." In fact, this implies that the consecutive referred to exists as an individual of a class, and even that it is unique, for otherwise we would say that the consecutives are *contained in* the class of integers.

nition of the individual is then justified.[41] But what we really demonstrate is not the existence of the individual as such but the existence of the class to which it belongs.[42]

In all of this there is no question of contradiction. What then is the relation between the existence of a class and the absence of contradiction in its definition? It consists in this: If a definition is contradictory, *no* individual fulfils its conditions, and consequently the corresponding class does not exist. Contradiction is then a purely negative criterion of existence; it is the criterion of non-existence. And reciprocally, if a class exists, that is to say contains an element, we can conclude from that, as M. Poincaré says, that its definition is not contradictory. Thus existence appears as the criterion of non-contradiction. But it is to be noticed that the relation between existence and contradiction is exactly the inverse of that which M. Poincaré affirms; it is not non-contradiction that proves existence, but it is existence that proves non-contradiction.[43]

It is then arbitrary and misleading to maintain that a definition is only valid if we first prove that it is not contradictory. Besides, it would be interesting to know how we could prove *directly* that a definition or a system of postulates is not contradictory. The presence of a contradiction can certainly be proved, but the absence of every contradiction is, like the innocence of an accused person, a negative fact which cannot be proved directly. Hilbert

[41] It must be noticed that, though the definition of a class has no need of justification (as this class may be null) the definition of an individual must be justified by the double demonstration of the *existence* and *uniqueness*. There is, then, no contradiction here.

[42] Cf. Russell, "The Existential Import of Propositions," *Mind*, July, 1905.

[43] After having written these lines, I found the same doctrine stated by G. Frege in his *Grundlagen der Arithmetik*, §§ 94, 95 (1884). "A concept is admissible, even when its marks contain a contradiction; only we must not suppose that anything falls in its extension. But from the mere fact that a concept does not contain a contradiction, we cannot conclude that something falls in its extension...." (§ 94). "The non-contradiction of a concept can only be established rigorously if we prove that something falls in its extension. The inverse would be an error" (§ 95).

UMI

stated in 1900 that we can find a direct demonstration of the compatibility of the axioms of arithmetic;[44] and in 1904 he believed that he had found such a demonstration.[45] But this demonstration is not satisfactory, in the opinion of M. Poincaré himself. If "M. Hilbert hides himself," it is not "because the difficulty is too great," but because the problems which he has proposed to himself appear insoluble. M. Padoa[46] has already replied to Hilbert by recalling that, in his own theory of algebraic numbers,[47] he has demonstrated, by the exemplary method which is the only one possible, the irreducibility of his postulates and their reciprocal independence. And he concluded with this phrase: "The contradictions or the dependencies of propositions can only be demonstrated by deductive reasoning while non-contradiction or independencies of propositions can only be demonstrated by verifications (we verify that properly chosen interpretation of the symbols satisfy or do not satisfy the propositions in question)." In fact, a contradiction or a dependence is translated by a proposition of non-existence or by an implication; while a non-contradiction or an independence is translated by a proposition of existence or by a non-implication. And this difference is equivalent to that of universal and particular propositions in classical logic. We know that we can only establish really universal propositions by demonstration, but that to establish a particular proposition it is enough to cite a single case in which it is true. In general we have no other means, for we cannot deduce it from universal premises without the adjunction of some particular proposition.

[44] Communication to the second international congress of mathematicians at Paris in 1900; cf. *Bulletin of the Amer. Math. Soc.*, 1902.

[45] Communication to the third congress at Heidelberg in 1904; cf. *Monist*, July, 1905. [Hilbert's paper is reprinted on pp. 263-279 of the third edition of his *Grundlagen der Geometrie*, published at Leipsic and Berlin in 1909].

[46] "Le problème no. 2 de M. D. Hilbert," *L'Enseignement mathématique*, Vol. V, 1903, pp. 85-91.

[47] *Bibliothèque du (1er) Congrès int. de Philosophie*, Vol. III, pp. 309-365; *Revue de mathématiques*, Vol. VII, 1901, pp. 73-84.

Now, just as it is impossible to deduce a particular from universal premises—that is to say a negation from many affirmations—it is impossible to prove deductively an existence or a non-implication if we set out from non-existences or from implication. Thus the direct method that Hilbert and Poincaré recommend is impracticable. M. Poincaré has no right, then, to require of the logistician a demonstration which Hilbert could not furnish. He might just as well convict them of impotence by summoning them to take a bite out of the moon.

In default of a direct demonstration M. Poincaré suggests a very curious method of verification. To prove that a system of postulates is not contradictory, it would be necessary, according to him, to compare two by two all their consequences to prove that "there are not two which are contradictory to each other." But, as he himself immediately recognizes, this method is impracticable if the consequences to be examined are infinite in number, as is the case in arithmetic. I add that, in fact, it has never been applied. Nobody has ever seen a mathematician spend his time in comparing among themselves all the propositions of a theory to assure himself that the definitions from which he started do not contain some contradiction, and that *consequently* the entities defined really exist. Where would we be if we had to make such a verification for each new definition? But, it would be replied, it is the whole of mathematics which constitutes this verification; it is a fact that no contradiction between any two propositions has ever been met. Very well, but this verification *a posteriori* is as valid for logistics as for mathematics, since logistics merely claims to formulate the primitive principles and definitions of mathematics. For example, we may ask if the postulates by which whole number is defined are not contradictory. Logistics has only to reply: I deduce from them all the theorems of classical arithmetic; you have

never found the least contradiction in these theorems when you made them rest on vague and confused intuitions; why do you wish that there should be any more contradiction in them at the present time? They are the same propositions, merely reduced deductively to some principles. In any case, the burden of proof falls on those who believe that these principles are contradictory; for contradiction may be proved, but non-contradiction may not.

The method in question is not only practicably inapplicable and unapplied in fact: it is logically illegitimate. In fact, it is not enough to bring two propositions together, to discover that they are contradictory, unless the contradiction is formal and explicit. For example, there is no formal contradiction between the two propositions: "ABCD is a non-rectangular parallelogram" and "ABCD is a quadrilateral which can be inscribed in a circle"; the contradiction only appears when we know the properties of the inscribable quadrilateral, that is to say, when the consequences of the second proposition are deduced. To bring to light the implicit and latent contradiction of two postulates, it would be necessary, accordingly, to deduce all the possible consequences (in number infinite) from these postulates. That presupposes the following definition: "Two propositions are contradictory to one another when they have consequences which are contradictory to one another." But such a definition is illogical because it contains a circle. Thus M. Poincaré's criterion of non-contradiction implies, not merely an infinite regress, but also a vicious circle.

M. Poincaré well knows that the method which he proposes is impracticable. He tries to correct it by means of the principle of induction: "Perhaps there may be a means of showing that a new reasoning cannot introduce contradiction, provided that we suppose that, in the series of preceding reasonings, we have hitherto met with none." Notice the very doubtful form in which this hypothesis is

stated. Indeed, it is a hypothesis in the air, which rests on no example and on no precedent, and which seems to be invented merely to charge the logisticians with a vicious circle. Now, not only is it not true, that is to say, nobody has ever used so strange a kind of reasoning, but it is improbable. To show this, let us see what further indications M. Poincaré gives. He supposes that "a series of syllogisms" can be formed from the starting-point of the axioms as premises; then, "when we have finished the nth syllogism, we see that we can make a $(n+1)$th"; lastly, we can show that, "if there has been no contradiction at the nth syllogism, there will not be at the $(n+1)$th." All these hypotheses are absolutely gratuitous and contrary to all probability. In the first place, mathematical reasonings do not, in general, consist in a *linear* series of syllogisms; otherwise the type of mathematical reasoning would be the *sorites*. Must we repeat that the syllogism is by no means the only type of deduction, and that there are many other logical principles or rules which enter into reasoning? Then, the simple deductions which compose a reasoning to not arrange themselves in a linear series, as M. Poincaré imagines; the image of mathematical reasoning is not a chain but rather a genealogical tree.[48] What, then, does the *number* of reasonings made at a given moment signify if their linear order is always more or less arbitrary, and arises solely from the practical necessity of enunciating them in speech, because time has only one dimension? "The number n serves to count a series of successive operations," says M. Poincaré; what becomes of his argument if these operations are not successive or are only so by accident? Can we affirm that this number n exists at each instant and that it is well determined? We can count simple deductions if they all reduce to the type of the syllogism; but

[48] Cf. *Les Principes*, p. 286 [and the long note on p. 238 of M. Couturat's article, which we have not here translated. It contains the detailed writing out of a simple theorem in the mathematical logic of Peano].

how are we to count heterogeneous deductions which proceed from various rules? Will it be said that each application of a logical principle constitutes a unity? But, besides the fact that all the principles have not the same deductive importance, it can happen that many principles occur at once in an elementary deduction. That is what happens, notably, when one intervenes as premise and the other as a rule of deduction. All this proves that the *number* of deductions, whether syllogistic or not, has no objective reality, and that any numbering of them is arbitrary. Consequently, how are we to admit that a proposition depending on this number can be established and concluded from n to $n+1$? And then M. Poincaré relies on his hypothetical case to attribute to the logisticians a vicious circle which they have not committed. "With an *if*," says common sense, "we could put Paris in a bottle." It is with an *if* that M. Poincaré arrives at attributing a paralogism to the logisticians.

Unfortunately, M. Poincaré seems to forget elsewhere all his *if*'s when he asserts categorically that the principle of induction is necessarily used in every demonstration of the compatibility of the axioms of arithmetic or of any system of axioms. For example, he says: "We must have recourse to processes of demonstration where, in general, we have to use the same principle of complete induction which is the one to be verified." Would it not be believed that the fantastic method which he proposes is in current use? Elsewhere he cites it as one of the "possible applications of the principle of induction," as if this application had been actually made. Finally he says, on the subject of the theorem of Bernstein: "If ever another demonstration is invented, it must still rest on this principle, since the possible consequences of the axioms which are to be shown to be non-contradictory are infinite in number." Thus, it is enough that we have to do with an infinity of propositions

(or of any objects) in order that, according to M. Poincaré, the principle of induction *necessarily* intervenes. He has quite forgotten that the application of this principle proposed by him is subject to extremely restrictive hypotheses.

At the bottom, he seems to confuse mathematical induction with induction pure and simple. For how are we to conceive that from the absence of contradiction in a series of reasonings we can infer the absence of contradiction in the following reasonings? Without doubt, if this inference was certain and could be expressed by the precise formula: "If no contradiction has been found in the first n reasonings it will not be found in the $n+1$ first ones," there would be an occasion to apply the principle of induction, and the conclusion would be equally certain. But the inference in question can at most only be probable, and consequently it only constitutes a common induction and not a mathematical induction. To borrow an example from M. Poincaré, the geometry of Lobachevski, since it only comprises a finite number of theorems, did not absolutely prove that the postulate of Euclid is independent of the other geometrical axioms (that is to say that its negation is compatible with them); it only gave this proposition a probability which was greater as the number of theorems of the new geometry became greater. But there is always an abyss between a probability, however great it may be, and an apodictic certainty. Now, the results of common induction are characterized by probability, while mathematical induction is a rigorous process which engenders certainty. If then the inference that is drawn from reasonings already made to future reasonings has only a probable value (as common sense—that "sure instinct" to which M. Poincaré refers—thinks), it rests on an induction pure and simple and not on the principle of mathematical induction.[49]

[49] [The fourth section of M. Couturat's paper occupies pp. 241-247, and contains a detailed refutation of M. Poincaré's remark that Mr. Russell had not demonstrated the existence of the integers. M. Poincaré's opinion rested

The second principal fault with which M. Poincaré charges the logisticians consists in that they surreptitiously change a definition: "You give a subtle definition of number and then you think nothing more about it. . . . and when the word 'number' is found farther on, you attach the same meaning to it as the first comer would. . . . Here is a word of which we have given an explicit definition A. We then make use of it, in discourse, in such a way that it implicitly supposes another definition B." That is a very serious reproach that must not be urged without proof against logicians so rigorous and so practised as Peano and his collaborators. Now M. Poincaré gives no proof and confines himself to general reflections on method which affect logisticians less than anybody else, for there is continually in these reflections a question of "words" and of "phrases." Mathematicians who reason with words and phrases are doubtless liable to attribute to a term, instead of the meaning assigned to it by its definition, the meaning which current use gives it. But it is exactly to avoid these illogical associations and implications that the logisticians use symbols whose meaning is solely determined by their formal relations, and which are manipulated in virtue of formal rules of calculation. Has M. Poincaré already forgotten that he reproached logisticians with reducing reasoning to a blind mechanism, that is to say, with neglecting the meaning of their symbols? "To demonstrate a theorem it is not necessary nor even useful to know what it means"; "the mathematician has no need of understanding what he

partly on a misreading and partly on the fact that, in M. Couturat's popular book, the question of existence was rather neglected in comparison with Mr. Russell's work. However, in Mr. Russell's early work, while *existence* was treated at length, the far more important question of *entity* was not considered. Thus the justification of Mr. Russell's early existence-theorems does not now appear to be quite satisfactory, and accordingly is here left untranslated. The second part of the fourth section is also untranslated here. It contains a refutation of M. Poincaré's hasty judgment that the principle of induction is not the definition of finite number, and is slightly more technical than the rest of M. Couturat's paper. What follows is, in essentials, M. Couturat's fifth section.]

does." The two reproaches are contradictory; let M. Poincaré leave to the logisticians at least the advantage of "the logical correction of reasonings" which compensates for its "formal" and almost "unintelligent" character. In any case, all the general and vague reasons which he alleges to support his criticism return against it, for they tend to prove that the logisticians are exempt from the causes of error which he points out.

I have long sought in the articles of M. Poincaré for the precise proofs of his accusation. I believe that I have found one, and yet I am not quite sure. M. Poincaré reproaches Mr. Russell with using two different formulae of the principle of induction, and with confusing them illegitimately: "A number may be defined by recurrence; on this number we may reason by recurrence: these are two distinct propositions. The principle of induction does not teach us that the first is true, it teaches us that the first implies the second." He says again: "The principle of induction does not mean that every whole number can be obtained by successive additions; it means that, for all the numbers that can be obtained by successive additions, we can demonstrate any property by recurrence." In the first place, the expression "successive additions" is not precise. The question necessarily arises, "How many additions?", and the reply is, "a finite number"; but the finite numbers are characterized by the principle of induction. Consequently, M. Poincaré's proposed enunciation means: "For all the numbers which can be defined by recurrence (or by complete induction), we can demonstrate a property by recurrence." Now that is a wholly analytical proposition, and almost a tautology: "All the numbers which verify the principle of induction verify the principle of induction." If this were the formula of the principle of induction, it would be an analytic judgment, and not a synthetic judgment as M. Poincaré maintains.

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But that is not the true formula of the principle of induction, and it is incomprehensible how a mathematician like M. Poincaré could have made such a mistake. It is not with him just an airy remark, for he returns to this important question at the end of his second article and gives precise expression to his thoughts in the following terms: "A whole number is that which can be obtained by successive additions, it is that which can be defined by recurrence.... A whole number is that on which we can reason by recurrence....The two definitions are not identical; without doubt they are equivalent, but they are so in virtue of a synthetic *a priori* judgment; we cannot pass from one to the other by purely logical processes."[50]

Will it be said that the logisticians have invented a new enunciation of the principle of induction, which they set up against the classical enunciation? By no means, they have only translated the traditional enunciation into symbols. And what is still stronger, M. Poincaré himself quoted this traditional enunciation at the beginning of his first article: "We know the enunciation of this principle. If a property is true of a number 1,[51] and if we establish that it is true of $n+1$ provided that it is true of n, it will be true of all the whole numbers." Now that is one of the verbal translations of the formula of the principle of induction.[52] M. Poincaré cannot then dispute the exactness of the symbolic formula. Thus he accuses the logisticians of surreptitiously changing a definition; and it is he himself who, in one and the same article, changes the definition, or rather the enunciation, of the principle of induction!

[50][M. Couturat, on p. 249 of his article, formulates these two definitions in symbols, and shows that the passage from one to the other is effected by a process as analytic as the passage from the proposition, "Pompey is one of the x's such that Cæsar conquered x," to the proposition, "Pompey was conquered by Cæsar," or, "Cæsar conquered Pompey."]

[51] Or of the number 0; that comes to the same thing here.

[52] It is one of the verbal translations that I have given in *Les Principes*, p. 55.

To prove his accusation he himself commits the paralogism which he wrongly attributes to them, and all his reproaches of illogicality fall on himself alone. If I had the wit of M. Poincaré, I would say that his "adventure" is quite as instructive as that of M. Burali-Forti, and that it ought to "warn" the adversaries of logistics of the necessity of being circumspect.

I will not bring up the conclusion of the articles of M. Poincaré because I do not see the utility of carrying the discussion into history where it is complicated by questions of interpretation. The controversy is not "between Kant and Leibniz,"[53] but between M. Poincaré and the logisticians. Besides, the question, as M. Poincaré has put it, is not a question of general philosophy or of epistemology, but of pure logic. Admitting the principles and the primitive ideas of the logisticians, M. Poincaré has maintained that, setting out from these data, they cannot build up mathematics without another postulate—an appeal to intuition or a synthetic *a priori* judgment; and he has thought that he has discovered in their *logical* construction certain paralogisms (beggings of the question or vicious circles). I believe that I can conclude from the above discussion that not one of these theses is proved, and that, in particular, the logisticians have not committed any of the logical errors that are so lightly imputed to them. I have too high an idea of the wit and the character of M. Poincaré not to believe that he will form a more just and more favorable opinion of logistics...when he has studied it.

LOUIS COUTURAT.

PARIS, FRANCE.

[53] [*Monist*, April, 1912, Vol. XXII, p. 256.]

THE LATEST EFFORTS OF THE LOGISTICIANS.[1]

THE logicians have attempted to answer the preceding[2] considerations. For that, a transformation of logistic was necessary, and Russell in particular has modified on certain points his original views. Without entering into the details of the debate, I should like to return to the two questions to my mind most important: Have the rules of logistic demonstrated their fruitfulness and infallibility? Is it true they afford means of proving the principle of complete induction without any appeal to intuition?

THE INFALLIBILITY OF LOGISTIC.

On the question of fertility, it seems M. Couturat has naive illusions. Logistic, according to him, lends invention "stilts and wings," and on the next page: "*Ten years ago,* Peano published the first edition of his *Formulaire.*" How is that, ten years of wings and not to have flown!

I have the highest esteem for Peano, who has done very pretty things (for instance his "space-filling curve," a phrase now discarded); but after all he has not gone further nor higher nor quicker than the majority of wingless mathematicians, and would have done just as well with his legs.

On the contrary I see in logistic only shackles for the inventor. It is no aid to conciseness—far from it, and if

[1] Translated by George Bruce Halsted.

[2] "The New Logics," in *The Monist,* April, 1912.

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twenty-seven equations were necessary to establish that 1 is a number, how many would be needed to prove a real theorem? If we distinguish, with Whitehead, the individual x, the class of which the only member is x and which shall be called l x, then the class of which the only member is the class of which the only member is x and which shall be called ll x, do you think these distinctions, useful as they may be, go far to quicken our pace?

Logistic forces us to say all that is ordinarily left to be understood; it makes us advance step by step; this is perhaps surer but not quicker.

It is not wings you logisticians give us, but leading-strings. And then we have the right to require that these leading-strings prevent our falling. This will be their only excuse. When a bond does not bear much interest, it should at least be an investment for a father of a family.

Should your rules be followed blindly? Yes, else only intuition could enable us to distinguish among them; but then they must be infallible; for only in an infallible authority can one have a blind confidence. This, therefore, is for you a necessity. Infallible you shall be, or not at all.

You have no right to say to us: "It is true we make mistakes, but so do you." For us to blunder is a misfortune, a very great misfortune; for you it is death.

Nor may you ask: Does the infallibility of arithmetic prevent errors in addition? The rules of calculation are infallible, and yet we see those blunder *who do not apply these rules*; but in checking their calculation it is at once seen where they went wrong. Here it is not at all the case; the logicians *have applied* their rules, and they have fallen into contradiction; and so true is this, that they are preparing to change these rules and to "sacrifice the notion of class." Why change them if they were infallible?

"We are not obliged," you say, "to solve *hic et nunc* all possible problems." Oh, we do not ask so much of you.

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If, in face of a problem, you would give *no* solution, we should have nothing to say; but on the contrary you give us *two* of them and those contradictory, and consequently at least one false; this it is which is failure.

Russell seeks to reconcile these contradictions, which can only be done, according to him, "by restricting or even sacrificing the notion of class." And M. Couturat, discovering the success of his attempt, adds: "If the logicians succeed where others have failed, M. Poincaré will remember this phrase, and give the honor of the solution to logistic."

But no! Logistic exists, it has its code which has already had four editions; or rather this code is logistic itself. Is Mr. Russell preparing to show that one at least of the two contradictory reasonings has transgressed the code? Not at all; he is preparing to change these laws and to abrogate a certain number of them. If he succeeds, I shall give the honor of it to Russell's intuition and not to the Peanian logistic which he will have destroyed.

THE LIBERTY OF CONTRADICTION.

I made two principal objections to the definition of whole number adopted in logistic. What says M. Couturat to the first of these objections?

What does the word *exist* mean in mathematics? It means, I said, to be free from contradiction. This M. Couturat contests. "Logical existence," says he, "is quite another thing from the absence of contradiction. It consists in the fact that a class is not empty." To say: a's exist, is, by definition, to affirm that the class a is not null.

And doubtless to affirm that the class a is not null, is, by definition, to affirm that a's exist. But one of the two affirmations is as denuded of meaning as the other, if they do not both signify, either that one may see or

touch *a*'s which is the meaning physicists or naturalists give them, or that one may conceive an *a* without being drawn into contradictions, which is the meaning given them by logicians and mathematicians.

For M. Couturat, "it is not non-contradiction that proves existence, but it is existence that proves non-contradiction." To establish the existence of a class, it is necessary therefore to establish, by an *example*, that there is an individual belonging to this class: "But, it will be said, how is the existence of this individual proved? Must not this existence be established, in order that the existence of the class of which it is a part may be deduced? Well, no; however paradoxical may appear the assertion, we never demonstrate the existence of an individual. Individuals, just because they are individuals, are always considered as existent....We never have to express that an individual exists, absolutely speaking, but only that it exists in a class." M. Couturat finds his own assertion paradoxical, and he will certainly not be the only one. Yet it must have a meaning. It doubtless means that the existence of an individual, alone in the world, and of which nothing is affirmed, cannot involve contradiction; in so far as it is all alone it evidently will not embarrass any one. Well, so let it be; we shall admit the existence of the individual, "absolutely speaking," but nothing more. It remains to prove the existence of the individual "in a class" and for that it will always be necessary to prove that the affirmation, "Such an individual belongs to such a class," is neither contradictory in itself, nor to the other postulates adopted.

"It is then," continues M. Couturat, "arbitrary and misleading to maintain that a definition is valid only if we first prove it is not contradictory." One could not claim in prouder and more energetic terms the liberty of contradiction. "In any case, the *onus probandi* rests upon those who believe that these principles are contradictory." Pos-

tulates are presumed to be compatible until the contrary is proved, just as the accused person is presumed innocent. Needless to add that I do not assent to this claim. But, you say, the demonstration you require of us is impossible, and you cannot ask us to jump over the moon. Pardon me; that is impossible for you but not for us, who admit the principle of induction as a synthetic judgment *a priori*. And that would be necessary for you, as for us.

To demonstrate that a system of postulates implies no contradiction, it is necessary to apply the principle of complete induction; this mode of reasoning not only has nothing "bizarre" about it, but it is the only correct one. It is not "unlikely" that it has ever been employed; and it is not hard to find "examples and precedents" of it. I have cited two such instances borrowed from Hilbert's article. He is not the only one to have used it and those who have not done so have been wrong. What I have blamed Hilbert for is not his having recourse to it (a born mathematician such as he could not fail to see a demonstration was necessary and this the only one possible), but his having recourse without recognizing the reasoning by recurrence.

THE SECOND OBJECTION.

I pointed out a second error of logistic in Hilbert's article. To-day Hilbert is excommunicated and M. Couturat no longer regards him as of the logistic cult; so he asks if I have found the same fault among the orthodox. No, I have not seen it in the pages I have read; I know not whether I should find it in the three hundred pages they have written which I have no desire to read.

Only, they must commit it the day they wish to make any application of mathematics. This science has not as sole object the eternal contemplation of its own navel; it has to do with nature and some day it will touch it. Then

it will be necessary to shake off purely verbal definitions and to stop paying oneself with words.

To go back to the example of Hilbert: always the point at issue is reasoning by recurrence and the question of knowing whether a system of postulates is not contradictory. M. Couturat will doubtless say that then this does not touch him, but it perhaps will interest those who do not claim, as he does, the liberty of contradiction.

We wish to establish, as above, that we shall never encounter contradiction after any number of deductions whatever, provided this number be finite. For that, it is necessary to apply the principle of induction. Should we here understand by finite number every number to which by definition the principle of induction applies? Evidently not, else we should be led to most embarrassing consequences. To have the right to lay down a system of postulates, we must be sure they are not contradictory. This is a truth admitted by *most* scientists; I should have written *by all* before reading M. Couturat's last article. But what does this signify? Does it mean that we must be sure of not meeting contradiction after a *finite* number of propositions, the *finite* number being by definition that which has all properties of recurrent nature, so that if one of these properties fails—if, for instance, we come upon a contradiction—we shall agree to say that the number in question is not finite? In other words, do we mean that we must be sure not to meet contradictions, on condition of agreeing to stop just when we are about to encounter one? To state such a proposition is enough to condemn it.

So, Hilbert's reasoning not only assumes the principle of induction, but it supposes that this principle is given us not as a simple definition, but as a synthetic judgment *a priori.*

To sum up:

A demonstration is necessary.

The only demonstration possible is the proof by recurrence.

This is legitimate only if we admit the principle of induction and if we regard it not as a definition but as a synthetic judgment.

THE CANTOR ANTINOMIES.

Now to examine Russell's new memoir. This memoir was written with the view to conquer the difficulties raised by those Cantor antinomies to which frequent allusion has already been made. Cantor thought he could construct a science of the infinite; others went on in the way he opened, but they soon ran foul of strange contradictions. These antinomies are already numerous, but the most celebrated are:

1. The Burali-Forti antinomy;
2. The Zermelo-König antinomy;
3. The Richard antinomy.

Cantor proved that the ordinal numbers (the question is of transfinite ordinal numbers, a new notion introduced by him) can be ranged in a linear series, that is to say that of two unequal ordinals one is always less than the other. Burali-Forti proves the contrary; and in fact he says in substance that if one could range *all* the ordinals in a linear series, this series would define an ordinal greater than *all* the others; we could afterwards adjoin 1 and would obtain again an ordinal which would be *still greater,* and this is contradictory.

We shall return later to the Zermelo-König antinomy which is of a slightly different nature. The Richard antinomy (*Revue générale des sciences,* June 30, 1905) is as follows: Consider all the decimal numbers definable by a finite number of words; these decimal numbers form an aggregate E, and it is easy to see that this aggregate is

countable, that is to say we can *number* the different decimal numbers of this assemblage from 1 to infinity. Suppose the numbering effected, and define a number N as follows: If the *n*th decimal of the *n*th number of the assemblage E is

0, 1, 2, 3, 4, 5, 6, 7, 8, 9

the *n*th decimal of N shall be:

1, 2, 3, 4, 5, 6, 7, 8, 1, 1

As we see, N is not equal to the *n*th number of E, and as *n* is arbitrary, N does not appertain to E and yet N should belong to this assemblage since we have defined it with a finite number of words.

We shall later see that M. Richard has himself given with much sagacity the explanation of his paradox and that this extends, *mutatis mutandis,* to the other like paradoxes. Again, Russell cites another quite amusing paradox: *What is the least whole number which cannot be defined by a phrase composed of less than a hundred English words?*

This number exists; and in fact the numbers capable of being defined by a like phrase are evidently finite in number since the words of the English language are not infinite in number. Therefore among them will be one less than all the others. And, on the other hand, this number does not exist, because its definition implies contradiction. This number in fact is defined by the phrase in italics which is composed of less than a hundred English words; and by definition this number should not be capable of definition by a like phrase.

ZIGZAG THEORY AND NO-CLASS THEORY.

What is Mr. Russell's attitude in presence of these contradictions? After having analyzed those of which we have just spoken, and cited still others, after having given

them a form recalling Epimenides, he does not hesitate to conclude: "A propositional function of one variable does not always determine a class." A propositional function (that is to say a definition) does not always determine a class. A "propositional function" or "norm" may be "non-predicative." And this does not mean that these non-predicative propositions determine an empty class, a null class; this does not mean that there is no value of x satisfying the definition and capable of being one of the elements of the class. The elements exist, but they have no right to unite in a syndicate to form a class.

But this is only the beginning and it is needful to know how to recognize whether a definition is or is not predicative. To solve this problem Russell hesitates between three theories which he calls

A. The zigzag theory;

B. The theory of limitation of size;

C. The no-class theory.

According to the zigzag theory "definitions (propositional functions) determine a class when they are simple and cease to do so when they are complicated and obscure." Who, now, is to decide whether a definition may be regarded as simple enough to be acceptable? To this question there is no answer, if it be not the loyal avowal of a complete inability: "The rules which enable us to recognize whether these definitions are predicative would be extremely complicated and cannot commend themselves by any plausible reason. This is a fault which might be remedied by greater ingenuity or by using distinctions not yet pointed out. But hitherto in seeking these rules, I have not been able to find any other directing principle than the absence of contradiction."

This theory therefore remains very obscure; in this night a single light—the word zigzag. What Russell calls

the "zigzaginess" is doubtless the particular characteristic which distinguishes the argument of Epimenides.

According to the theory of limitation of size, a class would cease to have the right to exist if it were too extended. Perhaps it might be infinite, but it should not be too much so. But we always meet again the same difficulty; at what precise moment does it begin to be too much so? Of course this difficulty is not solved and Russell passes on to the third theory.

In the no-classes theory it is forbidden to speak the word "class" and this word must be replaced by various periphrases. What a change for logistic which talks only of classes and classes of classes! It becomes necessary to remake the whole of logistic. Imagine how a page of logistic would look upon suppressing all the propositions where it is a question of class. There would only be some scattered survivors in the midst of a blank page. *Apparent rari nantes in gurgite vasto.*

Be that as it may, we see how Russell hesitates and the modifications to which he submits the fundamental principles he has hitherto adopted. Criteria are needed to decide whether a definition is too complex or too extended, and these criteria can only be justified by an appeal to intuition.

It is toward the no-classes theory that Russell finally inclines. Be that as it may, logistic is to be remade and it is not clear how much of it can be saved. Needless to add that Cantorism and logistic are alone under consideration; real mathematics, that which is good for something, may continue to develop in accordance with its own principles without bothering about the storms which rage outside it, and go on step by step with its usual conquests which are final and which it never has to abandon.

THE TRUE SOLUTION.

What choice ought we to make among these different theories? It seems to me that the solution is contained in a letter of M. Richard of which I have spoken above, to be found in the *Revue générale des sciences* of June 30, 1905. After having set forth the antinomy we have called Richard's antinomy, he gives its explanation. Recall what has already been said of this antinomy. E is the aggregate of *all* the numbers definable by a finite number of words, *without introducing the notion of the aggregate E itself.* Else the definition of E would contain a vicious circle; we must not define E by the aggregate E itself.

Now we have defined N with a finite number of words, it is true, but with the aid of the notion of the aggregate E. And this is why N is not part of E. In the example selected by M. Richard, the conclusion presents itself with complete evidence and the evidence will appear still stronger on consulting the text of the letter itself. But the same explanation holds good for the other antinomies, as is easily verified. Thus *the definitions which should be regarded as not predicative are those which contain a vicious circle.* And the preceding examples sufficiently show what I mean by that. Is it this which Russell calls the "zigzaginess"? I put the question without answering it.

THE DEMONSTRATIONS OF THE PRINCIPLE OF INDUCTION.

Let us now examine the pretended demonstrations of the principle of induction and in particular those of Whitehead and of Burali-Forti.

We shall speak of Whitehead's first, and take advantage of certain new terms happily introduced by Russell in his recent memoir. Call *recurrent class* every class containing zero, and containing $n+1$ if it contains n. Call

inductive number every number which is a part of *all* the recurrent classes. Upon what condition will this latter definition, which plays an essential rôle in Whitehead's proof, be "predicative" and consequently acceptable?

In accordance with what has been said, it is necessary to understand by *all* the recurrent classes, all those in whose definition the notion of inductive number does not enter. Else we fall again upon the vicious circle which has engendered the antinomies.

Now *Whitehead has not taken this precaution.* Whitehead's reasoning is therefore fallacious; it is the same which led to the antinomies. It was illegitimate when it gave false results; it remains illegitimate when by chance it leads to a true result.

A definition containing a vicious circle defines nothing. It is of no use to say, we are sure, whatever meaning we may give to our definition, zero at least belongs to the class of inductive numbers; it is not a question of knowing whether this class is void, but whether it can be rigorously deliminated. A "non-predicative" class is not an empty class, it is a class whose boundary is undetermined. Needless to add that this particular objection leaves in force the general objections applicable to all the demonstrations.

* * *

Burali-Forti has given another demonstration.[3] But he is obliged to assume two postulates: First, there always exists at least one infinite class. The second is thus expressed:

$$u \epsilon \mathrm{K}\,(\mathrm{K} - \iota\Lambda)\,.\,\supset .\,u < v'u.$$

The first postulate is not more evident than the principle to be proved. The second not only is not evident, but it is false, as Whitehead has shown; as moreover any recruit would see at the first glance, if the axiom had been

[3] In his article "Le classi finite," *Atti di Torino,* Vol. XXXII.

stated in intelligible language, since it means that the number of combinations which can be formed with several objects is less than the number of these objects.

ZERMELO'S ASSUMPTION.

A famous demonstration by Zermelo rests upon the following assumption: In any aggregate (or the same in each aggregate of an assemblage of aggregates) we can always choose *at random* an element (even if this assemblage of aggregates should contain an infinity of aggregates). This assumption had been applied a thousand times without being stated, but, once stated, it aroused doubts. Some mathematicians, for instance M. Borel, resolutely reject it; others admire it. Let us see what, according to his last article, Russell thinks of it. He does not speak out, but his reflections are very suggestive.

And first a picturesque example: Suppose we have as many pairs of shoes as there are whole numbers, and so that we can number *the pairs* from one to infinity, how many shoes shall we have? Will the number of shoes be equal to the number of pairs? Yes, if in each pair the right shoe is distinguishable from the left; it will in fact suffice to give the number $2n-1$ to the right shoe of the nth pair, and the number $2n$ to the left shoe of the nth pair. No, if the right shoe is just like the left, because a similar operation would become impossible—unless we admit Zermelo's assumption, since then we could choose *at random* in each pair the shoe to be regarded as the right.

CONCLUSIONS.

A demonstration truly founded upon the principles of analytic logic will be composed of a series of propositions. Some, serving as premises, will be identities or definitions; the others will be deduced from the premises step by step.

But though the bond between each proposition and the following is immediately evident, it will not at first sight appear how we get from the first to the last, which we may be tempted to regard as a new truth. But if we replace successively the different expressions therein by their definition and if this operation be carried as far as possible, there will finally remain only identities, so that all will reduce to an immense tautology. Logic therefore remains sterile unless made fruitful by intuition.

This I wrote long ago; logistic professes the contrary and thinks it has proved it by actually proving new truths. By what mechanism? Why in applying to their reasonings the procedure just described—namely, replacing the terms defined by their definitions—do we not see them dissolve into identities like ordinary reasonings? It is because this procedure is not applicable to them. And why? Because their definitions are not predicative and present this sort of hidden vicious circle which I have pointed out above; non-predicative definitions cannot be substituted for the terms defined. Under these conditions *logistic is not sterile, it engenders antinomies.*

It is the belief in the existence of the actual infinite which has given birth to these non-predicative definitions. Let me explain. In these definitions the word "all" figures, as is seen in the examples cited above. The word "all" has a very precise meaning when it is a question of an infinite number of objects; to have another one, when the objects are infinite in number, would require there being an actual (given complete) infinity. Otherwise *all* these objects could not be conceived as postulated anteriorly to their definition and then if the definition of a notion N depends upon *all* the objects A, it may be infected with a vicious circle, if among the objects A are some indefinable without the intervention of the notion N itself.

The rules of formal logic express simply the properties

of all possible classifications. But for them to be applicable it is necessary that these classifications be immutable and that we have no need to modify them in the course of the reasoning. If we have to classify only a finite number of objects, it is easy to keep our classifications without change. If the objects are *indefinite* in number, that is to say if one is constantly exposed to seeing new and unforeseen objects arise, it may happen that the appearance of a new object may require the classification to be modified, and thus it is we are exposed to antinomies. *There is no actual* (*given complete*) *infinity.* The Cantorians have forgotten this, and they have fallen into contradiction. It is true that Cantorism has been of service, but this was when applied to a real problem whose terms were precisely defined, and then we could advance without fear.

Logistic also forgot it, like the Cantorians, and encountered the same difficulties. But the question is to know whether they went this way by accident or whether it was a necessity for them. For me, the question is not doubtful; belief in an actual infinity is essential in the Russell logic. It is just this which distinguishes it from the Hilbert logic. Hilbert takes the view-point of extension, precisely in order to avoid the Cantorian antinomies. Russell takes the view-point of comprehension. Consequently for him the genus is anterior to the species, and the *summum genus* is anterior to all. That would not be inconvenient if the *summum genus* was finite; but if it is infinite, it is necessary to postulate the infinite, that is to say to regard the infinite as actual (given complete). And we have not only infinite classes; when we pass from the genus to the species in restricting the concept by new conditions, these conditions are still infinite in number. Because they express generally that the envisaged object presents such or such a relation with all the objects of an infinite class.

But that is ancient history. Russell has perceived the peril and takes counsel. He is about to change everything, and, what is easily understood, he is preparing not only to introduce new principles which shall allow of operations formerly forbidden, but he is preparing to forbid operations he formerly thought legitimate. Not content to adore what he burned, he is about to burn what he adored, which is more serious. He does not add a new wing to the building, he saps its foundation.

The old logistic is dead, so much so that already the zigzag theory and the no-classes theory are disputing over the succession. To judge of the new, we shall await its coming.

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THE PHILOSOPHY OF RELATIVITY

IN THE LIGHT OF THE PHILOSOPHY OF SCIENCE.

Objectivity.

SINCE the dawn of civilization man has groped after truth. He has investigated it; he has pondered on it; he has made guesses and proposed hypotheses; he has approximated truth by allegories, foreshadowing it in verse and fable; and since he began to count and to measure he has reduced the results of his inquiry to exact statements.

All observations are necessarily subjective, but man is not satisfied with subjective truth, he wants objective truth and objectivity of statement is the ideal of science.

Is objectivity impossible? Must we abandon our ideal of science? It seems to us that science has more and more in its various fields approached its ideal of objective truth. Standard measures have been invented and perfected. Time is measured by a pendulum of definite size, even apparently trivial factors have been considered such as latitude and altitude; and our precision machines testify to the ingenuity of man's genius in his attempt to eliminate personal equations as much as possible. The reliability of scientific computation has reached a marvelous degree, but it is almost more astonishing that we are still dissatisfied and that our measurements of minute fractions of the wave lengths of light are no longer exact enough for our needs.

In the face of the enormous accomplishments of science in approximating the ideal of objectivity, a new school has

risen which goes so far as to deny all objectivity, and insisting upon the truth of relativity, it would make us believe that objectivity is a phantom.

The relativity principle was first pronounced by Einstein in the *Jahrbuch der Radioaktivität* (Vol. IV, pp. 411 ff., 1907). It was invented to account for certain difficulties in the explanation of optical and electrical phenomena by considering the relativity of the movements in a system that is not at rest, called a disturbed system in contrast to quiet systems. In all quiet systems the common laws of dynamics hold good and the proposition of the relativity principle has been made for the sake of accounting for the laws of disturbed systems.

The principle of relativity is an *a priori* postulate from which certain theorems are derived whose truth is to be verified or refuted by experiment. Mr. Norman Campbell says:[1]

"The principle is what is more often termed a 'theory' —that is to say, it is a set of propositions from which experimental laws may be logically deduced. It can be proved to be true or false in a manner convincing to everybody only by comparing the laws so deduced with those found experimentally; but a theory which never conflicted with experiment might yet (as I hold) be judged objectionable on other grounds, and, conversely, a theory which was not in complete accord with experiment might yet be judged satisfactory."

Among the postulates of the principle of relativity there is one, counted the second, which presents great difficulties. It proclaims that "The velocity of light determined by all observers who are not accelerated relatively to each other is the same whatever may be the relative velocities of the observer."

[1] See "The Common Sense of Relativity" in *The Philosophical Magazine* for April 1911, pp. 502 ff.

UMI

An unsophisticated thinker would naturally assume that the velocity of light must be expected to increase or decrease according to the velocity of the observer. But the relativist assures us that light is an exception; on his assumption light is like a shadow whose motion depends upon the motion of its body representing the observer. The relation of the shadow to its body remains the same, however its body's (the observer's) velocity may change.

The question as to the velocity of light is a question of physics, not of philosophy, and we will touch upon it later. Here we will state only that the main objection to the relativity principle is the inference which implicates our objective ideal of science.

Not all the relativists agree on all points of their doctrine, and contradictory statements are not uncommon. We can here only characterize the general tendency and will not enter into the individual interpretations too closely.

Relativists try to avoid a difficulty which we grant exists, but is not insurmountable. Idealists of former days have used more subtle methods to dispose of the belief in objectivity of things, of time, and of space. They have produced only quibbles and the relativists have succeeded no better; only it is strange that the movement has originated among the physicists.

In a former article[2] we have demonstrated the paramount importance of relativity, but for all that we see no necessity for abandoning the old ideal of science. On the contrary we feel inclined to insist on it more strongly than ever. We do not deny the relativity of all existence throughout and without exception, but we still cling to the old scientific ideal of objectivity and we can not see that the relativity principle, in the one-sided sense in which the relativity physicists uphold it, is well established.

Having discussed in the article mentioned the part

[2] "The Principle of Relativity," *Monist*, April, 1912.

which relativity plays in scientific method, we feel inclined to add a few suggestions concerning the significance of the recent movement among physicists who emphasize the principle of relativity and prophesy that through it a new era in the scientific interpretation of the world will have to begin.

We have seen that many of the paradoxes which are proclaimed by the relativity physicists disappear on close inspection, for the contradictions resolve themselves into purely verbal contrasts. The same object is not in itself longer or shorter, but the result of measurement will be different according to the conditions under which the measurements take place. And further, although time can be eliminated, although it may be treated as a function of space, or even be treated as a kind of fourth dimension, the conception of time will nevertheless still remain of great convenience. The truth is that we must subsume time and space under one common category which, with Kant and other thinkers of well-established classical tradition since the days of Aristotle, has been called "form." We must always bear in mind the interrelation between time and space and view the two as the forms of one and the same reality. Time is the form of doing, of progressive action, of change, of events, and space is the form of being, of existence in its juxtaposition of parts. The former is the order of procedure in which the latter is transformed. Neither can be thought without the other, and the two are one. The principle of simplicity requires us to consider both in their interrelation. But for all that the traditional notion of time still proves the best method for rendering measurements of changes intuitively clear while an elimination of time as proposed by the Relativity Physicists is apt to obscure the issue; and we come to the conclusion that experience has not without good reasons found in the proper terms "space" and "time" a very convenient, yea,

as it seems to me, the most appropriate, mode of representation.

It is strange that the relativity principle has been proposed for the very purpose of approximating objective truth with greater exactness, but instead of accounting for inexactness or inaccuracies in results and for apparent contradictions by taking into consideration the mistakes in calculation on account of the shifting conditions of this world which is a constant flux, a *panta rhei,* the leaders of the new movement cancel the old ideal of science which has guided us thus far and propose a new standard strongly tinged with subjectivism, built upon the basis of the relativity of all existence.

All experience is a mixture of objectivity and subjectivity: it is due to the interrelation between a sentient subject and the sensed objects. So far science has tried to eliminate the subjective side, the personal equation, while the relativity physicists deny the legitimacy of the ideal of objectivity, or as they call it, the concept of the real. It is true that in clinging to the facts of observation without trying to eliminate the subjective elements and thereby to unify our results in an objective statement, we simplify our calculations, but it is very doubtful whether this procedure can be generally applied to other than optical and electrical phenomena. Relativists deem the theory justified if they simplify their own line of labors. Mr. Campbell exclaims in his enthusiasm:

"Anything more beautifully straightforward it would be hard to conceive. Not only is the result magnificently simple, but it furnishes us with a mathematical instrument of extraordinary power. In place of the elaborate calculations which have hitherto been necessary in dealing with moving systems, all that we have to do now is to solve the problem under consideration for the limiting case of infinitesimal velocity, and then effect a mere algebraical trans-

formation. The only objection that seems likely to be raised is that the principle proves too much, that it appears impossible that such far-reaching conclusions can be drawn from such simple assumptions: the only difficulty, in fact, is that the thing is too easy."

"The crudest arguments based on the oldest theory of light lead to the conclusion that the rate of a clock as observed by a certain observer must change with the relative motion of clock and observer. For, it will be argued, the observer does not see the clock 'as it really is at the moment,' but 'as it was a time T earlier, where T is the time taken for light to reach the observer.' And on these lines it is easy to show that the apparent rate of a clock moving away from the observer with a velocity v is $(1 - v/c)$ times[3] the rate of the same clocks observed at rest. It is only the magnitude of the change concerning which the two theories differ.

" 'Yes,' says our objector, 'that is all very well: of course the apparent rate of the clock changes with motion, but does the real rate change?' We immediately inquire what the 'real rate' means. He is at first inclined to assert that it is the rate observed by an observer traveling with the clock, but when we inquire relative to what clock that observer is to measure the rate he becomes uneasy. He cannot compare another clock traveling with him, for if the 'real rate' of one clock has changed, so has the 'real rate' of the other; and he cannot use a clock which is not traveling with him, because he admits that he does not see such a clock 'as it really is.'

"Pressing our inquiries, I think we shall get an answer of this nature. 'If I take a pendulum clock to some place where gravity is different, the rate of the clock will change. It is a change of this nature which I call a change in the

[3] c denotes the universal velocity whatever it may turn out to be. See *ibid.* p. 508.

"real rate," and I want to know whether there is any change of that kind, on the theory of relativity, when the clock is set in motion.' Now why does our objector call a change of the first kind a change in the 'real rate'? The reply is to be found in the history of the word 'real.' The word is intimately associated with the philosophic doctrine of realism, which holds that the most important thing that we can know about any body is not what we observe about it, but its 'real nature,' which is something that is independent of observation.

Now, of course, a quantity which is wholly independent of observation cannot play any part in an experimental science, but there are quantities which are independent of observation in the more limited sense that they are observed to be the same by whatever observer the observation is made. The term 'real' has come to be transferred from the philosophical conception to such quantities. The 'real rate' of the clock is said to change when it is transferred to a place where gravitation is different, because all observers agree that the rate of the clock which has been moved has undergone an alteration relatively to that which has not been moved.

"Now in the conditions which we are considering the observers do not agree. If A and B, each carrying a clock with him, are moving relatively to each other, they will not agree as to the rate of either of their clocks relative to A's standard or to B's standard or to any other standard. The conditions which, in the case of the alteration of gravitation, gave rise to the conception of a 'real rate' are not present: in this case there is no 'real rate,' and it is as absurd to ask whether it has changed as it would be to ask a question about the properties of a round square. However, some people, who in their eagerness to escape the reproach of being metaphysicians have adopted without inquiry the oldest and least satisfactory metaphysical doctrines, are so

enamoured of the conception of 'reality' that they refuse to give it up. Finding that the observations of different observers do not agree, they define a new function of those observations, such that it is the same for all observers, and proceed to call this the 'real rate.' This function, according to the principle of relativity, is $\beta n'$ where n' is the rate of the clock as seen by an observer relative to whom it is traveling with the velocity v: according to that principle, if we substitute in that function the appropriate values for any one observer, the resulting number will always be the same. So far no overwhelming objection can be raised."

What the relativists call "real" we would call objective, and we deem the ideal of objectivity to be the goal of science. Mr. Campbell has much to say on the concept of reality:

"It is the great merit of the principle of relativity that it forces on our attention the true nature of the concepts of 'real time' and 'real space' which have caused such endless confusion. If we mean by them quantities which are directly observed to be the same by all observers, there simply is no real space and real time. If we mean by them, as apparently we do mean nowadays, functions of the directly observed quantities which are the same for all observers, then they are derivative conceptions which depend for their meaning on the acceptance of some theory as to how the directly observed quantities will vary with the motion, position, etc. of the observers. 'Real' quantities can never be the starting point of a scientific argument; by their very nature they are not quantities which can be determined by a single observation: the term 'real' has always kept its original meaning of some property of a body which is not observed simply.

"All the difficulties and apparent paradoxes of the principle of relativity will vanish if the attention is kept rigidly fixed upon the quantities which are actually observed. If

any one thinks he discovers that that principle predicts some experimental result which is incomprehensible, let him dismiss utterly from his mind the conception of reality. Let him imagine himself in the laboratory actually performing the experiment: let him consider the numbers which he will record in his note-book and the subsequent calculation which he will make. He may then find that the result is somewhat unexpected—to meet with unexpected results is the usual end of performing experiments,—but he will not find any contradiction or any conclusion which is not quite as simple as that which he expected.

"There is one further point sometimes raised in connection with the principle on which a few words may be said.

"It is sometimes objected that the principle 'has no physical meaning,' that it destroys utterly the old theory of light based on an elastic ether and puts nothing in its place, that, in fact, it sacrifices the needs of the physical to the needs of the mathematical instinct. That the statement is true there can be no doubt, but the absence of any substitute for the elastic ether theory of light may simply be due to the fact that the principle has been developed so far chiefly by people who are primarily mathematicians. It is well to ask, can any physical theory of light be produced which is consistent with the principle?

"The answer depends on what is meant by a 'physical theory.' Hitherto the term has always meant a 'mechanical theory,' a theory of which the fundamental propositions are statements about particles moving according to the Newtonian dynamical formulæ. In this sense a physical theory is impossible if the principle of relativity be accepted, for the same reason that a corpuscular theory of light is impossible, if the undulatory theory of light be accepted. Newtonian dynamics and the principle of relativity are two theories which deal in part with the same range of facts:

they both pretend to be able to predict how the properties of observed systems will be altered by movement. If they are not logically equivalent they must be contradictory: in either case an 'explanation' of one in terms of the other is impossible. It can be easily shown that they are contradictory: if the principle of relativity is true, Newtonian dynamics must be abandoned."[4]

We start with "the facts of observation," and try to establish the objective state of things, called also "the real"; but relativists ignore the latter, and since every observer has his own particular observation, they declare that there is neither real time nor real space. The real is ruled out from observation.

Suppose, however, that the clocks which the relativist observes were the heartbeats of the relativist himself and the observer were the diagnosing physician, would the relativist insist that the physician had better drop out of sight the notion of reality, that there is as little sense in asking for "the real rate" of his heartbeat as it is absurd "to inquire whether, if all triangles had four sides, all circles would be square"?[5] If we can not attain an absolutely correct objective statement, we keep at least the ideal in view and this ideal is not an empty dream.

The relativity principle is a mathematical view of certain problems worked out for the sake of most minute measurements; and the attitude of the relativists is stern. If the facts can not be clearly represented by it, the worse for the facts, and if the physicists declare that their physical theories are incompatible with it, a new brand of physicists has to be manufactured who will inaugurate a relativist reform in physics.

[4] This conclusion is reached by Sommerfeld in a paper, *Ann. d. Phys.*, XXXIII, p. 684, etc. (1910).

[5] See Campbell, *loc, cit.*, p. 509. The comparison is not appropriate.

Primary Concepts.

The relativity problem would never have originated had the philosophy of science been clearly and distinctly understood by physicists, but they have familiarized themselves very little with even the problems, let alone reached proper solutions which explain the elementary concepts of our scientific terms, the difference between substance and form, between energy and matter, and the significance of the purely formal sciences.

As mathematicians are in the habit of starting with axioms, so the relativists begin with postulates and these postulates come in collision with the primary concepts such as have been formulated among the orthodox physicists and mathematicians of the present day.

A truly scientific view will brook neither axioms in mathematics, nor postulates in philosophy, nor primary concepts in physics.

There has been much talk about primary concepts, and arguments have been offered why time is not a primary notion or why we should let it pass as such. The truth is that time as well as space are two methods of describing definite relations. Time is not so much a fourth dimension of space, though we might look upon it as if it were such, time is the measure of motion and space is the scope of motion. Both time and space are presupposed in the idea of motion. There is no time in itself, there is no space in itself. What Newton and others with him call absolute space is "space conception" and what they call absolute time is "time conception." Such are the ideas which by pure deduction on *a priori* arguments, physicists form of time and of space, just as mathematicians formulate the general conception of numbers, of distances and of other relations, angles, areas, etc.

The idea of primary concepts is a very unfortunate de-

vice to lay a foundation for science. The faults of this method will not show so long as specialists are concerned about specialist problems, but the carelessness of taking anything for granted shows itself as soon as any problem broadens out into a general inquiry when its connection with universal problems is questioned. Such primary concepts are assumed to be undefinable and self-evident. That opens the door to an arbitrary interpretation as to the nature of space and time and energy, and gives a wide berth to mysticism.

Science brooks neither axioms nor primary concepts. Science starts with experience; it quarries out of experience the stones of the purely formal sciences which furnish all the methods of both common sense knowledge and scientific inquiry. The most general characteristic of experience is activity. Activity manifests itself in change. Change implies motion; it means either change of place, i. e., moving from here to there, or change of combination, viz., a moving of particles among themselves. Change interferes with existing relations, it modifies the old interrelations and establishes new interrelations.

The nature of relations in one terse term is called form. The word "form" comprises both outer shape and inner structure, and all interrelations of things as well as thoughts can be determined by the laws of pure form, arithmetic, geometry, logic, etc. Under all circumstances change modifies relations and means "transformation." There is a transformation in the juxtaposition of things or their parts, and there is a succession of events. The scope of the former we call "space," of the latter "time"; or better from the former we deduce our notion of space, from the latter our notion of time.

Physical inquiry is not helped by calling certain features of experience "primary concepts" and least of all (as has been done) should space, time and force,—these

highly complicated constructions of *a priori* thought— be beclouded by this mystifying name. Both time and space are features of the form of existence, and force is a general term for that feature of existence which marks its activity as motion, viz., as change of place, or rather as that which causes changes and is measured by the resistance overcome.

If we adopt the relativist principle to ignore the scientific ideal of objectivity, i. e., if we define size as the result of measurement and moments of time as determinations of measurement by units of duration, without regard to the ideal of coincidental happenings, and a common standard of time, we may produce incredible statements against which common sense rebels, and Professor Magie in his Presidential Address,[6] delivered before the Physical Society and Section B of the American Association for the Advancement of Science, at Washington, D. C. (December 28, 1911), says in comment thereof:

"A description of phenomena in terms of four dimensions in space would be unsatisfactory to me as an explanation, because by no stretch of my imagination can I make myself believe in the reality of a fourth dimension. The description of phenomena in terms of a time which is a function of the velocity of the body on which I reside will be, I fear, equally unsatisfactory to me, because, try I ever so hard, I can not make myself realize that such a time is conceivable.....I do not believe that there is any man now living who can assert with truth that he can conceive a time which is a function of velocity or is willing to go to the stake for the conviction that his 'now' is another man's 'future' or still another man's 'past.'

"One of the members of this society, recognizing our present inability to conceive of relative time, and conceiving our intuitions of space and time to be the result of

[6] Published in *Science*, February 23, 1912, pp. 281 ff:

heredity operating through many generations of men who lacked the light of relativity, once proposed to me that every one who could get even a glimmer of the notion of relative time should persistently exercise his mind therein and teach it to his students, in the hope that in a few generations the notion would emerge with the force of an intuition. It would not be fair to leave the impression that he was solemnly serious when he made this suggestion."

Form (i. e., relativity) is, as much as matter and energy, an ultimate generalization and may be called a fundamental concept (not a primary concept), and all the work of science is a tracing of transformations.

It is essential for the measurement of space and time to employ as measures uniform units, for space of distance and for time of duration. In the same way we need uniform units to measure force.

Besides a quantitative analysis of experience, there is a qualitative analysis which traces such transformations as build up parts into a higher unit, whereby through the interrelation or the interaction of the parts a new thing originates possessed of properties which are absent in the parts before their combination.[7]

The law of change is called causality. Cause is the motion which starts the process of transformation; effect is the result of the change; and reason is the general rule (formulated as a so-called law of nature) from which we understand why the cause must have this effect.[8]

The so-called law of the conservation of matter and energy is a deduction from the law of causality, which can be made as soon as we understand that all happenings are transformations, for if all changes are transformation, the

[7] See for instance the author's exposition of the nature of quality in *The Monist*, Vol. XV, p. 375. See also *Philosophy of Form*, p. 12.

[8] This has been repeatedly discussed, e. g., in the author's *Fundamental Problems*, pp. 79 ff.

amount of existence, its *that*, remains the same, only its form changes.

While investigating the several problems of our experience, scientists assume that they deal with real occurrences and thus they implicitly grant the *that* of existence, popularly denoted "matter" and "energy," viz., thingishness (or with a Latin term "reality") and actuality. The existence of ether is but an extension of the concept matter and so physicists have so far believed in the existence of ether; but the relativity physicists, in their anxiety to propound original ideas, deny the existence of ether. Says Prof. William Francis Magie in his above mentioned Presidential Address:

"The principle of relativity in this metaphysical form professes to be able to abandon the hypothesis of an ether. All the necessary descriptions of the crucial experiments in optics and electricity by which the theories of the universe are now being tested can be given without the use of that hypothesis. Indeed the principle asserts our inability even to determine any one frame or reference that can be distinguished from another, or, what means the same thing, to detect any relative motion of the earth and the ether, and so to ascribe to the ether any sort of motion; from which it is concluded that the philosophical course is to abandon the concept of the ether altogether. I may venture to say that in my opinion the abandonment of the hypothesis of an ether at the present time is a great and serious retrograde step in the development of speculative physics. The principle of relativity accounts for the negative result of the experiment of Michelson and Morley, but without an ether how do we account for the interference phenomena which made that experiment possible? There are only two ways yet thought of to account for the passage of light through space. Are the supporters of the theory

of relativity going to return to the corpuscles of Newton? .....There is choice only between corpuscles and a medium, and I submit that it is incumbent upon the advocates of the new views to propose and develop an explanation of the transmission of light and of the phenomena which have been interpreted for so long as demonstrating its periodicity. Otherwise they are asking us to abandon what has furnished a sound basis for the interpretation of phenomena and for constructive work in order to preserve the universality of a metaphysical postulate."

The concepts substance, i. e., matter or mass, and energy are ultimate generalizations as much as form, but they are very different from form. We could do without the words "matter" or "ether" by the use of some other indication to be introduced in our formulas which denotes reality; but that would not disprove the truth of the popular view, which describes every concrete bodily existence as material, nor is it likely that the old method of nomenclature will be rendered antiquated or erroneous.

We must not forget what matter means. Matter is a word which denotes that quality of objects which all of them have in common, viz., objectivity. An object is a thing that is objected to us, that offers us resistance, that impresses itself upon our existence and thereby affects our senses, and by objectivity we understand the general property of concrete existence, the *that* of experience, or its reality, viz., its thingishness. To deny the reality of the real, the thingishness of things, is as ridiculous as the opposite mistake, i. e., to think of reality, or objectivity, or of matter as a mysterious entity in itself. There is no reality *in abstracto*, for every *that* of existence is of a definite form which acts somehow, and the activity of things we call their actuality, or, as we call it in physics, energy.

The same problem presents itself in the domain of the phenomena of ether, i. e., of light and electricity. There

are some good reasons to assume that concrete matter has originated by a contraction or condensation of a more primitive substance which for all we know may prove to be the luminiferous ether, that thin substance which has been assumed to be the medium of light and electricity. If it is claimed by modern physicists that the principle of relativity disposes of the ether, that we no longer need it and can discard a belief in it as a superstition, that all physical phenomena can be accounted for without the assumption of an ether, we confront the same situation as in the theory of energetics, where the claim is made "There is no matter, all is energy."

The truth of this position, so far as we freely grant it, is this, that all scientific explanation describes the transformation of things; it traces the changes that take place according to the laws of form (mathematics and mechanics). In experience we are confronted with the fact that it is so, but the scientist inquires into the factors how it has become so, *how* it acts, and *how* it changes. By describing the *how* in formulas (so-called laws of nature) we denote the several factors with algebraic letters, such as $g =$ gravity, $t =$ seconds of time, $d =$ the distance traversed by a falling body and $v =$ the velocity of the fall, etc., and express their interrelation in equations, as

$$v = gt \text{ and } d = \tfrac{1}{2}gt^2.$$

By this method the essential features of natural phenomena are expressed in symbols, and he who has been initiated into the secret meaning of the symbols and the method of using them, will be able to predict the course of events if he is in possession of the necessary data.

What we here call with one word "essential" Kirchhoff characterizes in two words "most complete and most terse," or to use the common version "the most exhaustive and simplest." We deem our term preferable, and we under-

stand by "essential" all that which is efficient to produce the result, not more, not less.

We speak of the three laws of Kepler and of the condensed statements of the law of gravitation as "formulas," and this term truly expresses the nature of these generalized descriptions of certain types of uniformities. They are reductions of events to their purely formal (i. e., purely relational) conditions, and these purely formal conditions are the determinant (i. e., the causative) factors in all possible phenomena of a special type.

This is not a new truth. How old it is may be inferred from the Greek term "formal"[9] which in its etymology means "the causal" or "the causative" because the Greek philosophers describe the formal factors as efficient in causation.

When we have traced the essential factors of a certain type of changes, the scientist's work is finished. Whether mankind will ever be able to complete a scientific comprehension of the world in all its details, must be regarded as doubtful, but wherever science has succeeded in discovering the essential factors and has reduced them to formulas, we have been enabled to offer for every such phenomenon a satisfactory explanation.

This procedure affords us an insight into the reason why the course of a certain phenomenon must be so, why it can not be otherwise, and in this procedure the *that* is the basis, the *how* is the method of cognition. There is no explanation possible for the *that*, for the reality of the real, for the actuality of the fact; all explanations refer to the *how*. The *that* is a brutal fact, and the ultimate goal of science is the how, the answer being the formulation of laws of nature which explain to us by a use of the law of pure form that under given circumstances definite transformations will take place. Knowledge of the laws of na-

[9] τὸ αἰτιῶδες, derived from αἰτία = cause.

ture helps man to adapt himself to nature and also to adjust his surrounding natural conditions to himself.

In our explanation we can omit the *that* as a matter of course, for it is understood that reality is real. We can describe the purely formal relations only, which are the essential part of explanations. There is no sense in explaining the *that*. We have simply to state whether or not a formula covers actual facts, but to deny the *that* and say that there is only a how the world wags, but there is no world, seems to us a proposition that misconceives the situation.

We must not forget that such a word as substance, denoting here both "matter" and "ether" or existence in general, is a term that stands for objective reality. Ether is the *that* of the phenomena of electricity and light, as matter is the *that* of bodily objects, declaring that they are real, that they are concrete, and the term "substance" covers any kind of existence, it embraces both matter and ether or whatever the ultimate world-stuff may be called. There is no sense in denying their actuality, and all that may be meant by such a denial can only be either the redundancy of an express declaration that the formulas of physics refer to real processes, or a denial of ether or of matter as existences in themselves apart from their manifestations in definite configurations or modes of motion — a proposition which nowadays no one will seriously dispute.

A denial of the existence of substance (of matter and ether) is a purely verbal quibble. We might as well deny the existence of energy and declare that there is no energy, that there are only changes of place. The truth is that the faculty of existence which manifests itself in changes of place is called energy. We must not conceive of energy as something in itself.

* * *

I am told that my own view is the gist of the principle

of relativity, and if that be true, I would gladly hail a philosophy of relativity as another name for the philosophy of science. I have myself characterized the philosophy of science as a philosophy of form, and form denotes the relations in their totality. However, I would add that the system in which I have formulated this philosophy of science is simpler than the world-conception of the relativity physicists, besides it rests on a more solid foundation and is absolutely free from paradoxes.

While I deny that we can dispense with the idea of objectivity (be it called matter, or ether, or substance) I claim that we need make no mention of it in our formulas. In this sense we can dispense with the mention of ether. While I would not take the several paradoxes of time and space as serious and deny their objective truth, I grant that by a little confusion of thought in calling time or space relations the results of our different measurements, we can legitimately produce these paradoxes by exhibiting the inevitable discrepancies which originate through measurements from different standpoints as objective contradictions. Finally I consider it the ideal of a scientific philosophy to reduce all possible occurrences to relations, to resolve them into questions of form, to look upon them as transformations, and therefore I say that the ultimate aim of science is to describe everything in formulas. I see no objection to the relativist claim that this is a postulate of science. In fact, I deduce this postulate directly from my conception of reality which presents itself everywhere in our experience as transformation. Thus we would justify the principle of relativity on the basis of the old traditional basis of exact science.

The main claim of the relativists is based upon their simplification of the electromagnetic equations, and this is granted even by the adversaries of the principle of relativity. Professor Magie says:

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"It is surely true that if it were not for this demand of simplicity, immediately attainable and at present expressed in the electromagnetic equations, the chief incentive to the development of the theory of relativity would be wanting."

The one simplification of formulas is attempted by certain relativists by a generalization of time and space into a higher four-dimensional system, and they call it a four-dimensional space. We may note incidentally that Wagner's Parsival has anticipated the doctrine of relativity, for in his search he utters the mysterious words: "*Zum Raum wird hier die Zeit!*" (Into space here changeth time!) The relativists might as well have called their four-dimensional space a four-dimensional time. We abstain from giving it a name, but subsume time and space under one and the same category as "form" which enables us to view time and space as two inseparable factors of the cosmic system of interrelations, and we deem it wise to remember that they are different. If the relativity physicists have this in mind and do not mean ulterior mystifications, I would not hesitate to join their ranks on this point.

* * *

We may add one more comment about simplification. Logical possibilities are wider than actualized reality. Reality is one instance among many others which are not actualized. The fictions of fairy tales, of Gulliver's Travels, and of religious myth are instances of it. But in the domain of pure logic even actually absurd conditions parade as legitimate potentialities. Actual space has three dimensions, but metageometricians have invented more-dimensional spaces. Why not? We have in the construction of purely logical systems the undeniable right to generalize into the not actualized logical possibilities and mathematicians can not be restrained from building up a pangeometry. While Euclidean space is homaloidal, they may

create all kinds of curved spaces, which are all legitimate before the tribunal of pure logic, if they are but consistent in themselves. The main gain derived from such constructions which will naturally appear to the average man of average common sense as gratuitous, if not positively nonsensical, consists in rising to a higher level and understanding from this higher point of view the actualized reality better than if he remains on the *terra firma* of a limited sense-experience.

It might help our comprehension of causality as a transformation according to the laws of form to conceive the chain of causation as reversible, that the condition of causes are turned into effects and that the final factors that bring about the effect become the causes. This view has been humorously worked out by Fechner who for this purpose assumes that the pendulum of events will go on for a while in the direction it takes now, but the time will come when it will swing back. And then it will appear to us as quite natural and necessary that the decayed and waste material from fields and polluted rivers pass into our bodies and are changed in our bowels into juice to go forth from our mouths on the dinner table as lovely fruit or cheese, with bread and butter, and as roast venison or fish to go back and constitute useful parts in the revived animal. It would please us to see all this come about and the thought of the resurrection of the lamb under the butcher's knife would demonstrate that there is a purpose in the law of causation. We would be accustomed to the outcome and deem it natural. In fact some notions of an inverse world order in the golden age when the lamb will feed on the wolf, when the deer will hunt the hunter, when the rich shall be poor and the poor rich, when the miserable will be comforted while the fortunate will be tortured has now and then received serious support in the religious hopes of the disinherited classes of mankind, and we may find in the New

Testament an echo of this belief in those traditions which come down to us from Ebionite sources, the parables of the foolishness of the rich and the benediction of the poor. Dives goes to Hell while Lazarus is carried by angels to Abraham's bosom. Abraham says in Luke xvi: 25: "Son, remember that thou in thy lifetime receivedst thy good things, and likewise Lazarus evil things: but now he is comforted, and thou art tormented." No mention is made that Dives was wicked and that Lazarus was good; the only argument is that the other world must be reverted in its order.

A view of this kind which generalizes the mechanical constitution of the world and sees the possibility of an inverted causation, just as an engine may be reversed, may widen our comprehension and simplify our formulas of moral action, but we need not for that reason believe in its actualization. It is simply an instructive *lusus imaginationis,* an ingenious and helpful fiction—like our conception of four-dimensional space.

The mathematician who limits his studies to the Euclidean plane will understand his problems better if he becomes familiar with the theorems of stereometry, or if he views the figures of plane geometry as projections; or again if he regards a certain set of curves as conic sections. And further many problems of stereometry find a simpler formulation if viewed from the more comprehensive, though purely imaginary, view-point of a four-dimensional geometry. All this indicates that the simplifications of which the relativity physicists boast, may be (and I am inclined to believe that they are) very harmless. For all I can say, judging merely from the acceptance they have found, they must be true, but I can not see why they should be subversive of the scientific world-conception of the past.

A peculiar view of time which has been proposed in all seriousness, although common sense might consider it as

absurd, is the concept of time and space as consisting of discrete ultimate units. Do not our years, and days, and our hours too begin at definite moments? We become fifty or sixty years old suddenly with the beginning of a definite minute, According to this, time would run in jerks like the jumping second hands, and it would ultimately consist of infinitesimally small units of duration. Space also would be stippled and not continuous. Every motion would have to proceed in hopping from spot to spot, and the surface of a plane would be not unlike a half-tone picture which produces the impression of a continuous level but consists in reality of different dots more or less deeply tinged with ink. Such conceptions of time and space are quite conceivable although our classical and well-established views of both present them as continua. If space and time were actual entities endowed with positive qualities, if they were not merely potentialities of motion, a scope in which we move about, we could discover the nature of space by experiment. However, as they are constructions made in the abstract domain of anyness we should not refuse to consider seriously all kinds of propositions as to the nature of time and space.[10]

In comment on theories of this kind we would say that duration is continuous, but time consists of discrete units of duration; and again the scope of motion shows us an uninterrupted expanse while geometry exhibits definite lines

[10] The present number of *The Monist* contains an article on "Atomic Theories of Energy" by Mr. Arthur E. Bostwick, which will prove of great interest even to those who do not accept this theory. In comment we would say that Mr. Bostwick's defense of an atomic theory of energy is certainly true of definite amounts of energy, and his theory holds good also in his comparison of energy to amounts of money values deposited in a bank account. If deposits were made in specie, we could trace every dollar of a deposit. It is true we can not do so, but this we can not do only because no one cares to receive definite and individual coins, but is satisfied with money in any form. Therefore the bank is like a reservoir of water which receives and gives out water as it happens to come. The bank gives credit for amounts received and pays out amounts according to request. Thus the individual coin is lost sight of as the many drops of water are definite and concrete masses, and every dollar in a bank represents some concrete value somewhere.

of definite direction and of definite length. Geometrical space in its classical Euclidean form is not stippled, nevertheless every construction is particular. Geometrical points have no extension, but they possess a definite location, being determined, e. g., by two crossing lines. Thus space is not the totality of all points, but the totality of our scope of motion and anywhere in space points may be laid down. In a word: Time and geometrical space are constructions invented for the purpose of making measurements possible in a scope of potentialities.

Actual existence is always definite, pure forms however as well as purely formal thoughts, are always potential.

It seems as if the beginning of actuality must consist in establishing something that is limited and concrete. In this way it appears plausible that a potential world would be continuous as an ocean of pure ether might be, but an actual world ought to consist of a group of units, of atoms, of definite particular specks of existence endowed with definite amounts of energy, and we ought to be able to trace every definite amount of existence through all the changes which in the process of evolution it will undergo; and this ought to be true as regards every amount of both matter and energy.

SOME PHYSICAL PROBLEMS OF RELATIVITY.

The physical problems presenting themselves in the experiments which have become connected with the movement of relativity do not seem to have any direct bearing on the principle of relativity itself. Relations are of a purely formal nature and relativity therefore belongs to the same kind of knowledge as arithmetic, geometry and logic. Relativity can and must be applied to physics just as much as there is an applied mathematics, but as the Pythagorean theorem is independent from its applications

in experience, so applied relativity can neither establish nor refute the principle of relativity. This is true above all of the well-known and most important Michelson-Morley experiment.

The instrument made in Berlin by Schmidt & Haensch was so delicate that it was of no use in Berlin, and even when placed upon the foundation for the pier of the equatorial in the Astrophysical Observatory at Potsdam the fringe of interference rings disappeared by stamping upon the pavement at a distance of about 100 meters. Every detail of consequence was taken into consideration, not only the motion of the earth through the ether but also the motion of the whole solar system towards the constellation of Heracles. The expansion of the brass arms of the instrument through a change in temperature, and also the bending of the arms through rotation were duly considered and the difficulties arising therefrom met. A scale ruled on glass was employed in order to dispense with the micrometer screw which here proved useless. Yellow light was used, because its wavelength is least difficult to measure.

If the ether is at rest while the earth moves through it, the time required for light to pass from one point to another on the earth's surface would depend on the direction in which it travels. Two pencils of light that travel over paths at right angles to each other will interfere; the one traveling in the direction of the earth's motion will travel 0.04 of a wave length farther than it would have done were the earth at rest, while the other pencil at right angles to the motion of the earth would not be affected.

The results of Professor Michelson's experiment are negative. He found very small displacements in the fringes of his ray of light, so small that they must be accounted as mere errors of the experiment. While we ought to expect

a displacement of 0.05 we have only such as lie between 0.004 and 0.015. Professor Michelson says:[11]

"The interpretation of these results is that there is no displacement of the interference bands. The result of the hypothesis of a stationary ether is thus shown to be incorrect, and the necessary conclusion follows that the hypothesis is erroneous.

"This conclusion directly contradicts the explanation of the phenomenon of aberration which has been hitherto generally accepted, and which presupposes that the earth moves through the ether, the latter remaining at rest."

In another article Professor Michelson states his result thus:[12]

"The luminiferous ether is entirely unaffected by the motion of the matter which it permeates."

Professor Michelson has varied the conditions of his experiment by trying whether deviations could be detected through a change of level, by throwing pencils of light upward and by repeating it at different hours of the day, but the displacements remained insignificant. One of Professor Michelson's articles ends thus:[13]

"In any case we are driven to extraordinary conclusions, and the choice lies between these three:

"1. The earth passes through the ether (or rather allows the ether to pass through its entire mass) without appreciable influence.

"2. The length of all bodies is altered (equally?) by their motion through the ether.[14]

[11] "The Relative Motion of the Earth and the Luminiferous Ether" in *The American Journal of Science,* Vol. CXXII, page 128.

[12] "Influence of Motion of the Medium on the Velocity of Light," in *The American Journal of Science,* Vol. CXXXI, page 386.

[13] "The Relative Motion of the Earth and the Ether," *The American Journal of Science,* Vol. CLIII, p. 478.

[14] This would be the case according to the theory of H. A. Lorentz, whose views are mainly presented in the *Encyclopädie der math. Wissenschaften.*

"3. The earth in its motion drags with it the ether even at distances of many thousand kilometers from its surface."

Another article by Professor Michelson on the same subject is published in *The American Journal of Science,* Vol. CXXXIV, p. 333.

What this famous experiment has to do with the principle of relativity except in a most general way, is not yet clear to those who have not joined the ranks of the relativity physicists; but the relativity physicists insist very vigorously and dogmatically that it proves, or at least favors, their theory. Professor Michelson himself has not joined their ranks, though he recognizes the difficulties of the situation.

It is strange that Michelson's experiment seems to stand in contradiction to another and older experiment made first by Bradley, which is known as the aberration of light. If the earth passes through the ether with its own velocity (e) while the rays of the sun come down upon the earth with the velocity of light (l) there ought to be a deflection of light amounting to e/l, viz., the velocity of the earth divided by the velocity of the light in its path from the sun towards the earth, and though this relation is very small, it has actually been observed and determined to amount to a trifle over twenty seconds.

This conclusion which could be anticipated according to the logic of mechanics seems to be contradicted by Michelson-Morley's experiment in which the attempt is made to measure with a ray of light the motion of the earth while passing through the ether.

The discrepancy between the two experiments will perhaps find a proper explanation in the proposition that if the source of light lies outside the earth as in the case of the rays of the sun, they will show the deflection. As is to be expected they would come down in straight lines like raindrops falling in an absolutely quiet air which

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would be caught by a moving body as if they came down at an angle; but if the source of light moves along with the earth there would be no difference whichever way they turn, first towards the east or first towards the west, or at right angles, and the sources of the light would partake of the acceleration of the earth so as to show no difference, as raindrops dripping down within the car would fall down in straight lines from its top to the floor, assuming that the doors and windows of the car are hermetically closed and there be no draft which would deflect their perpendicular dripping.

It almost seems as if some ether were carried along by the earth to a considerable distance beyond its surface while the other ether in outer space would remain at rest, but it would be bold for any one but a specialist to venture the proposition of any theory on so new a subject of which few facts only have been ascertained. Yet most assuredly the topic under investigation has nothing to do with the principle of relativity, unless relativity is a misnomer for the phenomena attributed to the luminiferous ether.

The question of relativity is a philosophical problem, but the Michelson-Morley experiment is of a purely physical nature, and so we must expect that the last word as to its explanation should be given by physicists.

The other experiment which is assumed to verify the principle of relativity is the one first made by Kauffmann, and afterwards repeated in a modified form by Bucherer. This experiment too has little or nothing to do with relativity. On the contrary it seems to prove the existence of something absolute for it reaches a limit of velocity.

There is at present a tendency in the world of thought, noticeable in pragmatism and other anti-intellectual movements, which seems to annihilate the very existence of objectivity, and with it science, man's endeavor after a

purely objective cognition. Everything is relative, and the general belief has spread that an absolutely objective description is impossible. To speak of the size of objects seems to have lost its sense, for size has become to the present generation merely the result of measurement, and thus an objective determination is in some quarters looked upon as a superstition of prescientific tradition, an inheritance from the dark ages. But it is not true that there is no objectivity, for one of the greatest accomplishments of Michelson was the establishment of a definite measure by calculating the size of a meter in wave-lengths or red cadmium light in a vacuum. The waves of light are absolutely definite, and thus we have here a result of measurement in truly objective terms. If the Kauffmann-Bucherer experiments prove, as is claimed, that an increase of velocity means an increase of mass and that the limit which is reached is the velocity of light, we only learn that relativity is not without bounds, and that on the contrary a climax is reached which can not be surpassed. The highest velocity is the velocity of light.

The conclusion that the highest velocity is the velocity of light seems to be contradicted by the facts of gravitation for according to the Newtonian theory gravitation is possessed of a practically infinite velocity in that the gravity of the sun exercises its influence upon the planets without any perceptible difference of time. But this is no objection, for consider: The action of gravity formulated in the well-known law of falling bodies and of their acceleration which describes true motions is very slow in comparison to the velocity of light. The influence which is exercised in the strain between two gravitating bodies, say between the moon and the earth, is not a motion at all, but a condition, and this condition is the same between the two centers of the thus interrelated bodies. It is a state of tension and there is no transference of a wave motion either from

the moon to the earth or from the earth to the moon. The tension is simultaneous. The misconception seems to rise from the error that there are two bodies and there is a third item which manifests itself as a passing from the one to the other under the name of gravitation. We must view the whole system as one field of action in which several bodies in motion are balanced among themselves according to their mass. Their mutual attraction is not transferred motion but a simultaneous interaction. Newton retarded the general acceptance of the law of gravitation, first definitely proposed by Hooke, for eighteen long years because he could not make up his mind to believe in an *actio in distans,* and when he was finally convinced, he still expressed his misgivings how to overcome this objection, but is there any *actio in distans* at all? Is not the whole system of the universe an interrelated whole and does not a center of gravity (howsoever it may have originated) extend so far as its stress reaches? Where its strain produces a tension, there it affects its surroundings. If we look upon the phenomena of gravitation in this light we need not make the fantastical assumption that gravity is possessed of an infinite velocity.

The relation between the increase of velocity and the increase of mass promises to throw light on the ultimate constitution of matter, but the result of the experiment is only the first step to a solution of this tremendous problem, concerning which at the present stage of science we can have only vague suggestions. When the man appears who can read the facts aright, he may be able to point out how by a mere stress the aboriginal world-stuff which, for all we know, may be, or even must be, the ether, produces a tension within this mysterious infinitely elastic and incredibly thin substance, and the tension between two centers of such contraction would, like the strain between nodes within thin tridimensional rubber, act in all direc-

tions according to the Newtonian formula of gravitation, as being directly proportional to the product of their amounts of contraction, and inversely proportional to the square of their distance between two centers. Thus the origin of matter would be due to an unknown force which with a velocity only inferior to the velocity of light would drive infinitely small corpuscles around in a whirling dance with such a regulated speed that conglomerated multitudes of such whirls would appear to us as solid masses.

Here again we would be confronted by an ultimate limit. We would discover that objective reality, our world of matter in motion, is built up of ultimate particles; or perhaps better, of ultimate activities, that below the atom there are smaller units, the hypothetical electrons, which may be characterized as centers of force, and that they are due to condensation which produces the phenomena of gravitation. All further phenomena of physics and chemistry would have to be explained as the result of these elementary actions.

Formerly thinkers were inclined to see infinity all around. They thought of the atomic structure not only as infinitesimally small, but also as truly infinite; the molecules being analyzable into atoms and the atoms again into still smaller units, say into electrons or monads, and that the monads were again compounds of monadules and so forth—all this being argued on the poetic notion that

> "Great fleas have little fleas
> Upon their backs to bite 'em,
> And little fleas have lesser fleas,
> And so *ad infinitum*."

The molecule is a kind of planetary system, with atoms as satellites, so is the atom with its circling electrons; why should not the electron be of the same construction and why should not the component parts of the electron be assumed to be made after the same pattern world without

end? On the other hand our solar system is one among uncountably many others of the Milky Way; and the Milky Way in its turn is one universe of an enormously larger system of many Milky Ways. This is the conclusion which astronomy has deduced from actual facts. Why then should not this in our opinion enormous system of the many Milky Ways be only a tiny item in a still larger system, and why should we not be justified in the assumption that we are confronted with an infinite vista into both directions toward the infinitely small and the infinitely great?

This notion has been brought out in the second quatrain which reads:

> "And the great fleas themselves in turn
> Have greater fleas to go on,
> While these again have greater still,
> And greater still and so on."

A vista into infinitudes, going out into the infinitely small and the infinitely great, now seems to become untenable, and definite limits loom up, which condition, so it seems to us, would reveal, not a bottomless and undefinable relativity but a definite world of an objective reality with definite interrelations and limits. If there are definite limits in either direction we may fairly well assume that they are in both directions. Further, if the universe is definite in its space relation, it should also be definitely limited in time. The world may have originated in an immeasurable ocean of uniformities as a definite commotion and may terminate again in a general dissolution by dissipation. If such be the case the relativity principle would not apply to the whole. Relativity would mean the interrelationship of all things, but the whole as a whole would be of a definite particularity with definite boundaries while the constitution of the world would exhibit a structure of extremely tiny ultimate units of a determinably definite size,

endowed with a definite velocity and at every given point of a definite form of motion.

While the totality of existence, the sum total of our Milky Ways, appears to have had a beginning and may after the lapse of immeasurable ages come again to an end, we do not deem it excluded that the same process of world-formation may start again, as it probably was repeated long before the origin of this our present universe. While thus everything existent, even the ether itself in its totality, would have to be regarded as particular and concrete with definite boundaries and as being limited to a definite time both in its beginning and in its end, there would after all loom up in the background of this world an infinitude of space, an eternity of time and an unfathomable wealth of potentialities as to new formations which in spite of all the light which the most advanced science will ever shed on the world problem will keep this great All of existence with its inexhaustible resources and its mysterious order an object of constant wonder and awe.

The relativity problem as such is a philosophical problem, but the relativity physicists have made a physical problem of it, and the philosophical problem of relativity is not a new problem, it is as old as science; it is only the lack of philosophical training which has led to the enunciation of some baffling paradoxes which if they were true would make objective science impossible, for they would abolish definiteness of any kind and do away with objectivity. And strange to say, claims of this kind are upheld on the ground of experiments which tend to establish the existence of an absolute, or as we would prefer to say, of some ultimate, which would prove that our experience does not float as a local tangle in an endless infinitude, but that there is a beginning and end, and also a boundary of all concrete reality at every definitely given moment. No mysticism is needed. Infinitude and eternality are potential-

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ities, not actualities. They are vistas of what may be, not what is. They constitute the inexhaustible wealth of nature and of life without robbing science of its validity.

There is a tendency in mankind to think of the present moment as the climax of the past, which ushers in a new era by being an unprecedented and unique start. Every new generation passes through such a period of self-sufficiency and of an intoxication with their own incomparable selfhood. The old problems seem new to them, and trying to formulate them in an original way, they applaud their own mistakes as something extraordinary and wonderful. Goethe characterizes this tendency in the young graduate who has just taken his degree of Bachelor (See *Faust*, Second Part, Act II) where this young man vents his ambitious conceit in these words:

> "This is Youth's noblest calling and most fit!
> The world was not, ere I created it;
> The sun I drew from out the orient sea;
> The moon began her changeful course with me;
> The Day put on her shining robes, to greet me;
> The Earth grew green, and burst in flower to meet me,
> And when I beckoned, from the primal night
> The stars unveiled their splendors to my sight.
> Who, save myself, to you deliverance brought
> From commonplaces of restricted thought?
> I, proud and free, even as dictates my mind,
> Follow with joy the inward light I find,
> And speed along, in mine own ecstasy,
> Darkness behind, and Glory leading me!"

It is apparent that the relativity physicists confront an important problem, but they have not succeeded in solving it; they have not even as yet properly formulated the question and their propositions are still in a state of fermentation. It is difficult to say what will come of it. It is to be hoped, however, that the movement will follow the usual course of mental growth. The relativists will drop their extravagant claims, they will mature the truth which they grope after and will at last formulate it into clear state-

ments so as to justify the prophecy of Mephistopheles, who comments upon the proud words of the young Bachelor thus:

> "Go hence, magnificent Original!—
> What grief on thee would insight cast!
> Who can think wise or stupid things at all,
> That were not thought already in the Past?
> Yet even from him we're not in special peril;
> He will, ere long, to other thoughts incline:
> The must may foam absurdly in the barrel,
> Nathless it turns at last to wine."

At the present state of our knowledge it would be fantastical to suggest a solution of the physical problems connected with the relativity movement, and we must leave the discussion of them to the future, for ere we can approach a solution we must know much more about the ultimate constituents of matter.

Who will furnish the key to the lock of the closed door at which the relativity physicists are knocking?

CONCLUSION.

The details of the physical problems and their solution have only a slight interest for philosophy. The philosopher, however, expects that the physicist's solutions shall be consistent and that our scientific world-conception shall tolerate no contradictions.

If we consider the all-importance of form and the enormous significance which the formal sciences possess, we are inclined to regard the philosophy of relativity as a synonym and parallel development of the philosophy of science — the philosophy of form. But before we can definitely say so, we would expect the relativists to work out their philosophical substructure in a conservative way, to rid themselves of their paradoxical propositions, give up false pretensions to originality, recognize the past traditions of science, and rather than abandon the past, join

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their cause to the legitimate progress that follows from the tendencies, the ideals and aspirations of the established sciences.

We do not deny the relativity of all existence throughout and without exception, but we still cling to the old scientific ideal of objectivity and we can not see that the relativity principle is well established.

The great question before the world of thinkers is this: Is it possible to construct a philosophy of science? The author of this essay has answered this question in the affirmative, and has worked in this field for fully a quarter of a century. He has worked out the details of a philosophy of science, and has submitted to the world in both *The Open Court* and *The Monist* his answers to the several philosophical questions. These questions are: the nature of the soul; the origin of sentiency and of thought; the nature of reason, especially in its origin and in its relation to language, the mechanism with which reason manifests itself; the nature of ethics and the foundation of morality as it is found in the laws of the objective world; the significance of the God-conception as the authority of conduct, as the ideal of right and wrong, as the standard of truth and error, as the object of devotion, of gratitude, of reverence mainly as the factor which determines good and evil. All these questions are not beyond the scope of scientific inquiry and in the philosophy of science definite solutions are propounded which, though based on radical principles of unbiased thought, lead to a justification of the historical growth of religion and science.

The whole scope of existence as it presents itself in human experience can become an object of scientific inquiry, and all scientific problems admit ultimately of a definite solution without equivocation or prevarication, yet at the same time science is only one attitude among several others from which the world can be confronted. The noetic

conception is the ideal of understanding the world in its pure objectivity represented in mental terms to the exclusion of sentimental subjectivity. But man is not a child of reason only. He is also endowed with sentiments, with will and with artistic tendencies. While the scientific world-conception is absolutely indispensable for the man of thought who works for a constant elevation of mankind upon a higher level, we must at the same time recognize the rights of the large masses who naturally are non-scientific and are swayed by sentiment, by devotion, by art, by ethical aspirations, by a religious comprehension of life; and thus we see in artistic and religious conceptions ways of treating the world problem which are by no means unjustified and ought not to be repudiated on the ground that they are non-scientific, sometimes unscientific, or even anti-scientific and purely sentimental. Religious cosmogonies, ecclesiastical ceremonies, religio-poetical fictions possess values of their own which can not and should not be measured by the standards of scientific method. The mystic also has his right to confront the world with his emotions and visions. Nevertheless, even here the philosophy of science will be capable of investigating various products of these tendencies and has a right to evaluate their truth or untruth by tracing the meaning of allegorical poetry as well as the wholesomeness of ethical attitudes which they encourage. In this way the philosophy of science as worked out by the present writer has by no means been narrow but has granted a free scope to all legitimate tendencies of the human mind, and if the philosophy of science has been properly understood, leaders of thought in the movements of pragmatism, relativism, Bergsonianism and other modern tendencies, would have been able to avoid at least some of their aberrations, and could have devoted their energies to efforts in the right direction. At any rate they would have been better understood; instead of being classified with

philosophy, they would more properly have been regarded as a new species of poetry, or as literary ebullitions. Such they are; as such they possess value. They are not philosophy, certainly not philosophy in the strict sense of the word; they are not scientific world-conceptions.

It may appear strange to class the movement which proclaims the principle of relativity in the same category with pragmatism and other antiscientific tendencies. We do so because the relativists have much in common with pragmatists, because both cancel the ideal of objectivity, both identify truth with the subjective conception of the real or with the observer's statement of facts. They identify size with result of measurement and think that the traditional view of truth is an error.

We do not overlook the fact that the relativists are of a highly intellectual type and employ scientific methods, but their aim is after all a denial of the old ideal of science, of the objectivity of truth, and of clearness of thought. All this is surrendered for the sake of a purely subjective simplification of statement which recommends itself in their own specialty. Certainly there is a great difference between relativists and pragmatists, but we recognize in both a subjectivist tendency and a subjectivist aim. Neither of them feel the need of approximating objectivity and both indulge in ideal constructions, both build air castles, the former of mathematical fiction, the latter of philosophical poetry.

All these modern anti-scientific isms may have originated through the one-sided tendencies of a misapplied scientism or even through the lack of comprehension of the principles and the significance of science among naturalists. These isms emphasize therefore certain contentions which have a nucleus of truth, by insisting on the rights of sentiment though they go too far when attacking science itself

and claiming a superiority for unscientific sentiment over clear and methodical thought.

There is no question that all these modern movements try each in its own way to satisfy legitimate tendencies, but in doing so they have mostly gone astray; partly they misunderstand their own aspirations, partly they lack sufficient depth of comprehension and width of horizon in encompassing the whole realm of human endeavor.

We do not expect that in this partisan scramble of various prejudices, the whole world of thinkers can be induced to recognize the common ideal of philosophical thought, but we hope that there will be enough minds to understand the several movements, to appreciate them so far as their aspirations are legitimate, and to discover their weak points in which they stray away from the straight path that leads forward to a truer, deeper and a broader conception of the world.

EDITOR.

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ATOMIC THEORIES OF ENERGY.

A THEORY involving some sort of a discrete or discontinuous structure of energy has been put forward by Prof. Max Planck of the University of Berlin. The various aspects of this theory are discussed and elaborated by the late M. Henri Poincaré in a paper entitled "L'Hypothèse des Quanta," published in the *Revue Scientifique* (Paris, Feb. 21, 1912).

A paper in which a discontinuous or "atomic" structure of energy was suggested was prepared by the present writer fifteen years ago but remains unpublished for reasons that will appear later. Although he has no desire to put in a claim of priority and is well aware that failure to publish would put any such claim out of court, it seems to him that in connection with present radical developments in physical theory the paper, together with some correspondence relating thereto, has historical interest. Planck's theory was suggested by thermodynamical considerations. In the paper now to be quoted the matter was approached from the standpoint of a criterion for determining the identity of two portions of matter or of energy. The paper is as follows:

SOME CONSIDERATIONS ON THE IDENTITY OF DEFINITE PORTIONS OF ENERGY.

It has been remarked recently that physicists are now divided into two opposing schools according to the way in

which they view the subject of energy, some regarding it as a mere mathematical abstraction and others looking upon it as a physical entity, filling space and continuously migrating by definite paths from one place to another. It may be added that there are numerous factions within these two parties; for instance, not all of those who consider energy to be something more than a mere mathematical expression would maintain that a given quantity of it retains its identity just as a given quantity of matter does. In fact a close analysis would possibly show that opinions are graded very closely and continuously from a view hardly differing from that of Lagrange, who clearly saw and freely used the mathematical considerations involving energy before the word had been invented or its physical meaning developed, up to that stated recently in its extreme form by Professor Ostwald, who would replace what he terms a mechanical theory of the universe by an "energetical" theory, and would dwell exclusively on energy as opposed to its vehicles.

Differences of opinion of this sort very frequently reduce to differences of definition, and in this case the meaning of the word "identity" or some similar word or phrase has undoubtedly much to do with the view that is taken of the matter. It may be interesting, for instance, to look for a moment at our ideas of the identity of matter and the extent to which they are influenced by the accepted theory of its constitution.

Very few persons would hesitate to admit that the matter that now constitutes the universe is identical in amount with that which constituted it one million years ago, and that any given portion of that matter is identical with an equal amount of matter that then existed, although the situations of the parts of that portion might be and probably were widely different in the two cases. To assert this is of course a very different thing from asserting that

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the identity of the two portions or any parts thereof could have been practically shown by following them during all their changes of location or state. That cannot be done even in the case of some simple changes that are effected in a fraction of a second. For instance, if water from the pail A be mixed with water from the pail B there is no possible way of telling which pail any given portion of the mixture came from or in what proportions, yet it is certain that such portion is identical with a portion of equal mass that recently occupied part of one or both pails.

How far our certainty as to this is influenced by our ideas regarding the ultimate constitution of the water is worthy of investigation. All who accept the molecular theory, for instance, will regard our inability to trace the elements of a mixture as due to purely physical limitations. A set of Maxwell's "demons" if bidden to watch the molecules of the water in pail A, one demon being assigned to each molecule, would be able to tell us at any time the precise proportions of any given part of the mixture. But if we should not accept the molecular theory and believe for instance, that water is a continuum, absolutely homogeneous, no matter how small portions of it be selected, then our demons would be as powerless as we ourselves now are to trace the constituents in the mixture.

We are now in a position to ask the question: Is the matter in a mixture of two continua identical with that of its constituents? The identity certainly seems of a different kind or degree from that which obtains in the first case, for there is no part, however small, that was derived from one pail alone. The mixture is something more than a mere juxtaposition of elements each of which has retained its identity; it is now of such nature that no part of it is identical with any part of A alone or of B alone, nor of A+B, where the sign + denotes simple juxtaposition. It is identical, to be sure, with a perfect mixture of certain

parts of A and B, but this is simply saying that it is identical with what it is now, that is, with itself, not with something that went before.

Probably no one now believes that water or any other kind of matter is a continuum, but the bearing of what has been said may be seen when we remember that this is precisely the present stage of our belief regarding energy.

No one, so far as I know, has ventured to suggest what may be termed a molecular theory of energy, a somewhat remarkable fact when we consider the control now exercised over all thought in physics by molecular theories of matter. While we now believe, for instance, that a material body, say a crystal, can by no possibility increase continuously in mass, but must do so step by step, the minimum mass of matter that can be added being the molecule, we believe on the contrary that the energy possessed by the same body can and may increase with absolutely perfect continuity, being hampered by no such restriction.

It is not the purpose of this paper to discuss whether we have grounds for belief that there is such a thing as a minimum quantity, or atom, of energy, that does not separate into smaller parts, no matter what changes it undergoes. Suffice it to say that there appears to be no *a priori* absurdity in such an idea. At first sight both matter and energy appear non-molecular in structure. But we have been forced to look upon the gradual growth of a crystal as a step-by-step process, and we may some day, by equally cogent considerations, be forced to regard the gradual increase of energy of an accelerating body as also a step-by-step process, although the discontinuity is as invisible to the eye in the latter case as in the former.

Without following this out any farther, however, the point may be here emphasized that it is hardly possible for one who, like the majority of physicists, regards matter as molecular and energy as a continuum, to hold the same

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ideas regarding the identity of the two. Efforts to show that definite portions of energy, like definite portions of matter, retain their identity have hitherto been made chiefly on the lines of a demonstration that energy travels by definite and continuous paths in space just as matter does. This is very well, but it would appear to be necessary to supplement it with evidence to show that the lines representing these paths do not form at their intersections continuous blurs that not only forbid any practical attempt at identification on emergence, but make it doubtful whether we can in any true sense call the issuing path identical with the entering one. Otherwise the identity of energy can be admitted to be only that kind of identity that could be preserved by matter if its molecular structure did not exist. One who can admit that this sort of identity is the same sort that can be preserved by molecular matter may be able to hold the identity of energy in the present state of the evidence, but the present attitude of physicists would seem to show that, whether they realize the connection of the two subjects or not, they cannot take this view. In other words, modern views of the identity of matter seem closely connected with modern views of its structure, and the same connection will doubtless hold good for energy.

Regarding the probable success of an attempt to prove that energy has a "structure" analogous to the molecular structure of matter, any prediction would doubtless be rash just now. The writer has been unable, up to the present time, to disprove the proposition, but the subject is one of corresponding importance to that of the whole molecular theory of matter and should not be entered upon lightly.

* * *

The writer freely acknowledges at present that the illustrations in the foregoing are badly chosen and some of the statements are too strong, but it still represents essen-

tially his ideas on the subject. No reputable scientific journal would undertake to publish it. The paper was then sent to Prof. J. Willard Gibbs of Yale, and elicited the following letter from him:

"NEW HAVEN, June 2/97.

"MY DEAR MR. BOSTWICK:

"I regret that I have allowed your letter to lie so long unanswered. It was in fact not very easy to answer, and when one lays a letter aside to answer, the weeks slip away very fast.

"I do not think that you state the matter quite right in regard to the mixture of fluids if they were continuous. The mixing of water as I regard it would be like this, if it were continuous and not molecular. Suppose you should take strips of white and red glass and heat them until soft and twist them together. Keep on drawing them out and doubling them up and twisting them together. It would soon require a microscope to distinguish the red and white glass, which would be drawn out into thinner and thinner filaments if the matter were continuous. But it would be always only a matter of optical power to distinguish perfectly the portions of red and white glass. The stirring up of water from two pails would not really mix them but only entangle filaments from the pails.

"To come to the case of energy. All our ideas concerning energy seem to require that it is capable of gradual increase. Thus the energy due to velocity can increase continuously if velocity can. Since the energy is as the square of the velocity, if the velocity can only increase discontinuously by equal increments, the energy of a body will increase by unequal increments in such a way as to make the exchange of energy between bodies a very awkward matter to adjust.

"But apart from the question of the increase of energy

by discontinuous increments, the question of relative and absolute motion makes it very hard to give a particular position to energy. Since the 'energy' we speak of in any case is not one quantity but may be interpreted in a great many ways. Take the important case of two equal elastic balls. One, moving, strikes the other at rest, we say, and gives it nearly all its energy. But we have no right to call one ball at rest and we can not say (as anything absolute) which of the balls has lost and which has gained energy. If there is such a thing as absolute energy of motion it is something entirely unknowable by us. Take the solar system, supposed isolated. We may take as our origin of coordinates the center of gravity of the system. Or we may take an origin with respect to which the center of gravity of the solar system has any (constant) velocity. The kinetic energy of the earth, for example, may have any value whatever, and the principle of the conservation of energy will hold in any case for the whole solar system. But the shifting of energy from one planet to another will take place entirely differently when we estimate the energies with reference to different origins.

"It does not seem to me that your ideas fit in with what we know about nature. If you ask my advice, I should not advise you to try to publish them.

"At best you would be entering into a discussion (perhaps not in bad company) in which words would play a greater part than precise ideas.

"This is the way I feel about it.

"I remain

"Yours faithfully,

J. W. GIBBS."

Professor Gibbs's criticism of the illustration of water-mixture is evidently just. Another might well have been used where the things mixed are not material—for instance

the value of money deposited in a bank. If A and B each deposits $100 to C's credit and C then draws $10, there is evidently no way of determining what part of it came from A and what from B. The structure of "value," in other words, is perfectly continuous. Professor Gibbs's objections to an "atomic" theory of the structure of energy are most interesting. The difficulties that it involves are not overstated. In 1897 they made it unnecessary, but since that time considerations have been brought forward, and generally recognized, which may make it necessary to brave those difficulties.

Planck's theory was suggested by the apparent necessity of modifying the generally accepted theory of statistical equilibrium involving the so-called "law of equipartition," enunciated first for gases and extended to liquids and solids.

In the first place the kinetic theory fixes the number of degrees of freedom of each gaseous molecule, which would be three for argon, for instance, and five for oxygen. But what prevents either from having the six degrees to which ordinary mechanical theory entitles it? Furthermore, the oxygen spectrum has more than five lines, and the molecule must therefore vibrate in more than five modes. "Why," asks Poincaré, "do certain degrees of freedom appear to play no part here; why are they, so to speak, 'ankylosed'?" Again, suppose a system in statistical equilibrium, each part gaining on an average, in a short time, exactly as much as it loses. If the system consists of molecules and ether, as the former have a finite number of degrees of freedom and the latter an infinite number, the unmodified law of equipartition would require that the ether should finally appropriate all energy, leaving none of it to the matter. To escape this conclusion we have Rayleigh's law that the radiated energy, for a given wavelength, is proportional to the absolute temperature, and for a given temperature

is in inverse ratio to the fourth power of the wave-length. This is found by Planck to be experimentally unverifiable, the radiation being less for small wave-lengths and low temperatures, than the law requires.

Still again, the specific heats of solids, instead of being sensibly constant at all temperatures, are found to diminish rapidly in the low temperatures now available in liquid air or hydrogen and apparently tend to disappear at absolute zero. "All takes place," says Poincaré, "as if these molecules lost some of their degrees of freedom in cooling —as if some of their articulations froze at the limit."

Plank attempts to explain these facts by introducing the idea of what he calls "quanta" of energy. To quote from Poincaré's paper:

"How should we picture a radiating body? We know that a Hertz resonator sends into the ether Hertzian waves that are identical with luminous waves: an incandescent body must then be regarded as containing a very great number of tiny resonators. When the body is heated, these resonators acquire energy, start vibrating and consequently radiate.

"Planck's hypothesis consists in the supposition that each of these resonators can acquire or lose energy only by abrupt jumps, in such a way that the store of energy that it possesses must always be a multiple of a constant quantity, which he calls a 'quantum'—must be composed of a whole number of quanta. This indivisible unit, this quantum, is not the same for all resonators; it is in inverse ratio to the wave-length, so that resonators of short period can take in energy only in large pieces, while those of long period can absorb or give it out by small bits. What is the result? Great effort is necessary to agitate a short-period resonator, since this requires at least a quantity of energy equal to its quantum, which is great. The chances are, then, that these resonators will keep quiet, especially if the

temperature is low, and it is for this reason that there is relatively little short-wave radiation in 'black radiation.'... The diminution of specific-heats is explained similarly: When the temperature falls, a large number of vibrators fall below their quantum and cease to vibrate, so that the total energy diminishes faster than the old theories require."

Here we have the germs of an atomic theory of energy. As Poincaré now points out, the trouble is that the quanta are not constant. In his study of the matter he notes that the work of Prof. Wilhelm Wien, of Würzburg, leads by theory to precisely the conclusion announced by Planck that if we are to hold to the accepted ideas of statistical equilibrium the energy can vary only by quanta inversely proportional to wave-length. The mechanical property of the resonators imagined by Planck is therefore precisely that which Wien's theory requires. If we are to suppose atoms of energy, therefore, they must be variable atoms. There are other objections which need not be touched upon here, the whole theory being in a very early stage. To quote Poincaré again:

"The new conception is seductive from a certain standpoint: for some time the tendency has been toward atomism. Matter appears to us as formed of indivisible atoms; electricity is no longer continuous, not infinitely divisible, it resolves itself into equally-charged electrons; we have also now the magneton, or atom of magnetism. From this point of view the quanta appear as *atoms* of *energy*. Unfortunately the comparison may not be pushed to the limit; a hydrogen atom is really invariable.... The electrons preserve their individuality amid the most divers vicissitudes, is it the same with the atoms of energy? We have, for instance, three quanta of energy in a resonator whose wave-length is 3; this passes to a second resonator whose wave-length is 5; it now represents not 3 but 5 quanta,

since the quantum of the new resonator is smaller and in the transformation the number of atoms and the size of each has changed."

If, however, we replace the atom of energy by an "atom of action," these atoms may be considered equal and invariable. The whole study of thermodynamic equilibrium has been reduced by the French mathematical school to a question of probability. "The probability of a continuous variable is obtained by considering elementary independent domains of equal probability.... In the classic dynamics we use, to find these elementary domains, the theorem that two physical states of which one is the necessary effect of the other are equally probable. In a physical system if we represent by q one of the generalized coordinates and by p the corresponding momentum, according to Liouville's theorem the domain $\int\int dpdq$, considered at a given instant, is invariable with respect to the time if p and q vary according to Hamilton's equations. On the other hand p and q may, at a given instant take all possible values, independent of each other. Whence it follows that the elementary domain is infinitely small, of the magnitude $dpdq$.... The new hypothesis has for its object to restrict the variability of p and q so that these variables will only change by jumps....Thus the number of elementary domains of probability is reduced and the extent of each is augmented. The hypothesis of quanta of action consists in supposing that these domains are all equal and no longer infinitely small but finite and that for each $\int\int dp\, dq = h$, h being a constant."

Put a little less mathematically, this simply means that as energy equals action multiplied by frequency, the fact that the quantum of energy is proportional to the frequency (or inversely to the wave-length as stated above) is due simply to the fact that the quantum of action is constant—a real atom. The general effect on our physical concep-

tions, however, is the same: we have a purely discontinuous universe—discontinuous not only in matter but in energy and the flow of time. M. Poincaré thus puts it:

"A physical system is susceptible only of a finite number of distinct states; it leaps from one of these to the next without passing through any continuous series of intermediate states."

He notes later:

"The universe, then, leaps suddenly from one state to another; but in the interval it must remain immovable, and the divers instants during which it keeps in the same state can no longer be discriminated from one another; we thus reach a conception of the discontinuous variation of time —the *atom of time*."

I quote in conclusion, Poincaré's final remarks:

"The present state of the question is thus as follows: the old theories, which hitherto seemed to account for all the known phenomena, have met with an unexpected obstacle. Seemingly a modification becomes necessary. A hypothesis has presented itself to M. Planck's mind, but so strange a one that one is tempted to seek every means of escaping it; these means, however, have been sought vainly. The new theory, however, raises a host of difficulties, many of which are real and not simply illusions due to the indolence of our minds, unwilling to change their modes of thought.....

"Is discontinuity to reign throughout the physical universe, and is its triumph definitive? Or rather shall we find that it is but apparent and hides a series of continuous processes?....To try to give an opinion just now on these questions would only be to waste ink."

It only remains to call attention again to the fact that this conception of the discontinuity of energy, the acceptance of which Poincaré says would be "the most profound revolution that natural philosophy has undergone since

Newton" was suggested by the present writer fifteen years ago. Its reception and serious consideration by one of the first mathematical physicists of the world seems a sufficient justification of its suggestion then as a legitimate scientific hypothesis.

ARTHUR E. BOSTWICK.

ST. LOUIS, MO.

CRITICISMS AND DISCUSSIONS.

HENRI BERGSON, PRAGMATISM AND SCHOPENHAUER.

The history of philosophy is like one of the ancestral galleries in ancient European castles. At first glance you find yourself before a bewildering variety of individuals, but if you look closer you discover that certain family traits reappear again and again. In the history of philosophy a similar variety of individual systems at first confuses the student. But upon closer scrutiny he will find that here too the bewilderment ceases, that certain outlines are typical for the structure of a great number of systems and that almost every individual system belongs to such a type of structure. Considered in this light the history of philosophy presents a few types of thought which undergo slight changes and show a slow development according to the intellectual conditions of the century in which the philosopher moulded his system.

The systematic structure to be considered in this paper is a very modern one, namely Henri Bergson's philosophy. From a purely philosophical standpoint it was severely criticized in the last number of this magazine.[1] In this number it may be scrutinized from a purely historical standpoint.

Among laymen Bergson's name carries with it a certain feeling of mystic refinement. However little they may know about him, they instinctively expect such an appeal to their artistic natures as they would from a sculpture by Auguste Rodin or a drama by Maeterlinck. They instinctively feel the kind of a man who is about to confront them and their feeling is probably correct.

Feeling, however, is quite a different thing from knowing. Strange to say, philosophers are greatly at variance as to the place of Bergson's philosophy in the gallery of philosophical systems.

[1] See "The Philosophy of Bergson" by Mr. Bertrand Russell, *Monist*, July, 1912. Cf. also in the same number, "Bergson and Religion" by Dr. James G. Townsend, and "Kant and Bergson" by Dr. Bruno Jordan.

Here is the struggle of a man's mind, some philosophers say, which can be compared in importance only with the philosophy of Kant, while still very different. Others are reminded of Hegel and others of Berkeley; while American pragmatists say that Bergson is a pragmatist. Others again are of the opinion that it is quite out of the question to compare Bergson at all with philosophers of any other school. They say he is unique; he is no type, but has an individuality of his own.

We shall see. It is certain however that it would be rather strange if Bergson were not a type, notwithstanding his marked individuality; if his philosophy really bore no relation to philosophies of former days. This would mean that in the development of philosophical traditions a structure of philosophy had arisen no likeness to which had ever been seen before, that a child without ancestors had been brought to light; but this is not very probable. To inquire therefore after the historical antecedents of Bergson's philosophy would mean to ask whether its structure is entirely new or only a transformation of what already existed, and in the latter case the questions arise, who used this structure before Bergson? Who among the philosophers of former days may be called nearest akin to him? Which philosophical tradition is continued by Bergson's thought? These are the questions which I shall try to answer in this paper.

* * *

Which philosophical tradition is continued by Bergson? "The pragmatistic one," say the American pragmatists; "Bergson is a pragmatist." I do not hesitate to agree that Bergson really is a pragmatist, and here are the proofs.

Pragmatism holds that what we call reality, world, object of knowledge, is not something independent of us, but rather a man-made picture, a raw material transformed into a complicated instrument for action. "The world is only an opportunity to do our duty," Fichte was wont to say. "The world is only an expedient for our action," the pragmatists of to-day tell us. Seeing, hearing, smelling, touching the world, as well as considering it as a multitude of atoms, electrons, ions, means nothing else than a preparation to grapple with the world.

This doctrine becomes very evident in Bergson's philosophy. For Henri Bergson the essence of man is life. What we call man is not so much the human body nor a soul-substance within that body, but rather a dramatic performance, a continual course of

events, of visions, sounds, feelings, images, thoughts and especially of actions. Man means an *élan vital*, a life-current running along seventy, eighty or ninety years.

If this is so, what is the business of that life-current? To what purpose is that action acting? In agreement with the biologists Bergson would answer that the business of life is to find its way among other life-currents. But if we look closer we shall see that these other life-currents differ widely from one another, that man treats them at least in very different ways, some of them as matter only and others as matter but also as souls.

For the present we shall disregard life-currents treated as souls. As far as their treatment as pure matter is concerned, Bergson is among the pragmatists. Man is different from most animals in that he has hands and uses them not only for climbing trees as the monkeys do but for providing instruments. It would not be too bold to say that man has made his hands longer and stronger by utilizing matter for instruments. The axe, the arrow, the target are such artificial hands, not to mention the enormous development of inventions during the last centuries, all tending to the same end.

This leads to the question of how this mechanical point of view influences our knowledge of reality. The answer is very simple. Since man intends to use reality for instrumental purposes it appears to him entirely as an object of action—either to act upon or to act with.

We seldom realize how much difference such a point of view makes in our knowledge. A desk in my study may serve as a simple instance. Everybody agrees that it is a desk. But "desk" means an *instrument* either to put a manuscript on, to read a book on, to fit a lamp on, etc. In other words, from the very beginning we all acknowledge a certain piece of wood as a *practical* object destined for *practical* purposes, and it would be surprisingly hard for us to free ourselves from this impression, to look at that object and not identify it with a desk. But desk means instrument and instrument means possibility of action. I shall return to this point very soon.

Meanwhile another reflection may serve to clear up this situation still further. I say that to all of us the desk appears as a desk. We cannot help it. Still the impression of the desk is by no means the same to all of us, for in addition to its being acknowledged as a desk it may be recognized in many other ways. Let me suppose

that a dealer in woods happens to examine my desk. He will see many features in it which other people do not observe. He will not only infer but actually see the quality of the wood and probably tell its cash-value off-hand; while all that other people see is that it is "apparently of oak" and possibly that it looks "rather nice." The merchant being a connoisseur actually sees more than other people. Now perhaps a carpenter looks at the desk, then an artist, then a botanist, then a chemist, then a physicist. The layman would simply laugh when the botanist says that the desk is a conglomeration of cells, or the physicist that it is a heap of molecules, or the chemist that it is a multitude of atoms. Still these are the ways in which the desk would be regarded by these men. It is only the *point of view* which is different.

In other respects however we all are taking the same point of view, for to all of us, whether layman, merchant or physicist, the desk appears as a desk, and if it does not appear as a desk, it would at least appear as a table. And if it does not appear as a table it would at least appear as a "thing," and "thing" always means, if we believe Bergson, *something to act upon or to act with.*

It is only the *point of view* which is different. In other respects however all are taking the same point of view, for to all of us, whether layman, merchant or physicist, the desk appears as a desk, or if not as a desk, at least as a table. And if it does not appear as a table it would at least appear as a "thing," and "thing" always means, if we believe Bergson, *something to act upon or to act with.*

Hence, in considering the world as an accumulation of things we have already taken a certain limited point of view; we are on the way to treat the universe as an object and eventually as a means of *action.* We have taken the decisive step, and cannot now go back. We have made the start in our calculation and it will proceed accordingly.

* * *

It is interesting that Bergson considers three-dimensional space as one of the most important elements in the method by which the world is conceived as a means for action. To see things in space means to consider them as objects to act upon. Space is a kind of uniform which we put upon the world in order to control it, for to control things we cannot care what they are in themselves; we must care what they may be to us. Hence we deprive them of their very essence; we treat them by a scheme, which enables us to divide things up quite at will—while according to Bergson things in them-

selves are indivisible, although, as he expresses it, they have a certain "ballast" of that scheme.

Our three-dimensional space is a scheme for division and nothing else; its very homogeneity is the means by which we divide things. This is most apparent in theoretical physics where all plurality of our pluralistic world is eliminated and only shadowy colorless sections of space remain where before we saw blue and red and green and yellow, where we heard noise and music, smelled odors and tasted sweet and bitter. Theoretical physics is space triumphant, which means it is a triumph of practical handling of the world. Plurality once eliminated from the world there is no limit for divisions. We may divide indefinitely, and dividing will doubtless contribute much toward our practical control over the world.

* * *

It is a very significant fact that we treat time in a similar way again and again; we spatialize it. If somebody knocks at the door three times in succession and we try to recall that succession of knocks, we discover in ourselves a certain inability to do so. Instead of representing the three knocks as a real sequence in time we find ourselves fancying them one beside or behind the other, quite as though they were three pearls on a cord or three blots on a line. We are almost compelled to do so; for to recall something means to see it all at once, and to see all at once is precisely not to see one part after the other in succession, but to perceive them simultaneously. Hence we spatialize time, and in so doing we are tending again towards its homogeneity. Of course we are not treating time as three-dimensional space—that would not do; but modern scientists treat it as a "fourth dimension" of space, and common sense regards it as a one-dimensional line. The next step in this procedure will be to eliminate all plurality of concrete experience from that line, to divide up time quite as arbitrarily as space and to deal with time too, regardless of its contents, as suits our purpose. The dial on a watch is nothing but a graphic demonstration of such a one-dimensional time-line, its straightness being turned into a circle.

Bergson's idea of the business of understanding is now sufficiently clear. Understanding is an instrument by which human life works its way through its surroundings. Moreover it is an instrument by which human life continually makes use of homogeneous schemes, treating simultaneous impressions by space and successive impressions by spatialized time. This threefoldness of understanding

—practical use, homogeneous space and spatialized time—may still become important for our later discussion. For the present it suffices to state that understanding is to Bergson an instrument for practical use. This is the reason why in America he is called a pragmatist.

* * *

And now in seeming contradiction to my own words, and in open contradiction to the theory of most American philosophers, I say that *Bergson is not a pragmatist.* He is the very *foe* of the pragmatists; for to tell the truth, what pragmatism advocates, Bergsonian philosophy *opposes.* It is one of the main features of this philosophy to *disregard* the whole realm of understanding as a realm for practical use. To Bergson's mind it is precisely the pragmatic attitude that *hinders* understanding from entering philosophy—and Bergson is after all a philosopher. He leaves to pragmatism the realm of science and common sense, but *in philosophy he protests against it.*

To Bergson's mind *philosophy begins where pragmatism ceases.* To be a Bergsonian philosopher I must rid myself of pragmatistic habits. It is pragmatistic to look at everything from the point of view of action, hence it is philosophic to discard that point of view. It is pragmatistic to consider the world as wrapped in space, hence it is philosophic to free the world from space. It is pragmatistic to spatialize time, hence it is philosophic to remove that spatialization.

A Bergsonian philosopher is a thinker freed from all pragmatism. He no longer looks for the practical use of things, but looks to things for their own sake. His mind no longer works to make headway for life, but it turns itself round and looks at life itself as it goes on within him. Bergsonian philosophy is consciousness of life itself freed from its practical service.

* * *

And now our thoughts do not leave Bergson in passing over to Schopenhauer and attempting to throw light on Bergson's ideas by the philosophy of Schopenhauer and on Schopenhauer's ideas by the philosophy of Bergson.

Like Bergson, Schopenhauer is a pragmatist; like the pragmatism of Bergson, that of Schopenhauer holds good for understanding only; and as for Bergson, so for Schopenhauer, philosophy begins just where understanding and pragmatism cease.[2]

[2] Cf. Schopenhauer, *Werke* (ed. Grisebach), I, p. 242.

Thus *understanding* plays quite a similar rôle with Schopenhauer as with Bergson. It is very significant that Bergson and Schopenhauer use the same simile. Both call understanding "a lantern" which life has kindled in order to find its way through the world. For both this "lantern," originally used for a limited service, has extended its light more and more until now it shines over the whole universe, or at least what it calls a "universe."

Now the "universe" over which this lantern sheds its light is matter. Our earth and all that lives on earth is matter. The moon, the sun, the planets, all solar systems are matter. But for Bergson as for Schopenhauer "matter" is nothing but reality treated by the methods of understanding, covered with three-dimensional space, put into spatialized time and considered pragmatically for the single purpose of action.

For Bergson matter is simply correlative to understanding; it is the only means by which understanding knows reality. Schopenhauer takes exactly the same position. "Matter," he writes, "and hence the appearance of the whole universe, is there for understanding only. Understanding is its support, the condition of its very existence; it is its necessary correlatum."[3]

It is important to realize that this coincidence between Bergson and Schopenhauer is not insignificant, but indicates a very interesting and far-reaching identity in the main structure of their systems. That understanding is limited to matter and matter limited to understanding is of decisive consequence for the whole development of both philosophies.

* * *

But the coincidence between Bergson's and Schopenhauer's idea of understanding goes much farther than this; for precisely the same *three functions of understanding* with regard to matter pointed out by Bergson are likewise pointed out by Schopenhauer. If Bergson declares that the only aim of understanding is to have the possibility of *acting* upon its environment, and that this is the reason why it *materializes* everything, Schopenhauer would say that *causality* is the only category of understanding, for understanding is there solely to *act* upon its environment. That is the service forced upon understanding by the all-powerful will to live, and that too is the main reason why understanding transforms everything into *matter*. In other words, Bergson's pragmatism of understanding and Schopenhauer's doctrine of causality as the only

[3] *Werke*, II, p. 160.

function of understanding tend precisely towards the same point, a point of greatest consequence, in fact a cornerstone in the structure of both systems.

For Schopenhauer as for Bergson understanding stands for activity and for activity only. Its functions to provide for *space* and *time,* are subordinate to that main function. They are the means by which understanding succeeds in materializing the world for the purpose of action.

When Bergson says that in order to handle the world for action understanding covers it with three-dimensional homogeneous space, Schopenhauer would say that three-dimensional homogeneous *space* is one of the two *indispensable intuitions a priori* of understanding. That space is *a priori* for Schopenhauer and possibly *a posteriori* for Bergson is of no consequence for this part of our comparison. But it certainly is of very great consequence that in the philosophy of both understanding uses space as its most important instrument in materializing the world for the purpose of action.

The other instrument by which understanding materializes the world is *time.* Bergson says that for this purpose time is spatialized. Schopenhauer declares that time is the other of the two *"intuitions a priori"* of understanding. Now there is apparently no similarity at all between the ideas of Schopenhauer and Bergson; and still, on looking closer, a careful observer will discover that there is a similarity, and indeed a far greater one than would be suspected. I venture to call special attention to this point.

What distinctive feature of space makes it appear to Bergson particularly adapted for the practical purpose of understanding? Certainly not that space is three-dimensional, but that three-dimensional space is *homogeneous.* Space is fit for action solely because of its homogeneity. With homogeneity arbitrary divisibility is possible, and with arbitrary divisibility understanding finds its way to handle reality instrumentally—that is all.

Let us pass to time. What distinctive feature of time makes it appear to Bergson as being particularly adapted for the practical purposes of understanding? Bergson finds this distinctive feature in the fact that we "spatialize" time. But what does he mean by spatializing time? Again nothing but an attempt to treat time like space, namely as a *homogeneous* something. It is not the one dimension nor the straightness of the line, that "spatializes" time; but its uniformity, its homogeneity, its divisibility at any point, and hence its possibility to be handled instrumentally. Homogeneity,

divisibility at any point, is the only quality for which understanding cares. It is wholly indifferent to everything else in space and in spatialized time.

Passing to Schopenhauer we discover that the reason why he, like his master Kant, considers space and time as "*intuitions a priori*" is again their all-embracing *homogeneity.* The reader remembers Kant's demonstration of this in his Critique of Pure Reason. Space and time have the peculiarity of not being "concepts," but "intuitions," because they are spread out with absolute uniformity, with absolute homogeneity, which does not allow the distinction between different (three-dimensional) "spaces." Furthermore these "intuitions" are *a priori* because they are necessary; they are necessary because they are supposed to cover everything; and they are supposed to cover everything once more because they are absolutely homogeneous—a uniform scheme which nothing can escape. Space and time are for Schopenhauer and Kant *intuitions a priori* in so far only as they are *homogeneous schemes,* to be placed over a world of immediate images.

Schopenhauer's idea of time, as an "*intuition a priori*" is the idea of a homogeneous scheme to be placed over a world of immediate images. But this is exactly what Bergson calls *spatialized time.* Therefore when Bergson contends that understanding as the correlative of matter and an instrument for action makes use not only of space but also of "spatialized time," and Schopenhauer contends that understanding as the correlative of matter and an instrument for action makes use not only of space but also of time as an "intuition *a priori*" they mean the very same thing. They only use a different vocabulary. It is essential for both that space and time are homogeneous schemes and belong to understanding as means of turning immediate images into matter and of handling matter practically. I shall very soon return to this point.

In the meantime I venture to sum up the result, thus far reached in our investigation, in the statement that the theories of Bergson and Schopenhauer with regard to understanding, matter, space and time are essentially the same. And since half of the entire structure of the philosophies of these two men is constituted by those theories, their sameness means a corresponding sameness of half that structure.

* * *

We now proceed to the second half of that structure. Opposed to understanding and matter there stands in Schopenhauer's philos-

ophy the "*will to live*." Opposed to understanding and matter there stands in Bergson's philosophy a "*life-current*," the "*élan vital*."

For Bergson as well as for Schopenhauer the world of understanding is only a world of "appearance." It is not true reality, but reality prepared for action. Hence, by understanding we are not in a position to penetrate into the essence of reality. We are getting only to the surfaces. We go around things, but cannot enter them.

We previously noted that Bergson used a simile originally used by Schopenhauer. Both compare understanding to a "lantern" which life has kindled in order to find its way through the world. Now we discover for a second time that Bergson and Schopenhauer use one and the same simile in precisely the same way. Schopenhauer writes with regard to the inability of understanding to enter life: "We see that it is *impossible to reach the inner essence of things by external means*; however much we thus investigate we find only images and empty names. We are like the man who walks around a castle, looks in vain for an entrance into it and in the meantime *sketches its walls*."[4] Compare with this Bergson's account of understanding trying in vain to enter life: "It is like the work of an artist," he writes, "who traveling through Paris *sketches a tower* of Notre Dame.... This designer *replaces the true inner essence of the object by an external schematic reproduction*."[5]

It is the way of understanding to approach its objects by external means only, and in doing so it tries to comprehend the life inherent in them by dividing the object up into small parts. This, however, is in vain. Understanding can never enter life in this way, for the only true way of coming into contact with life is by feeling it instinctively.

Schopenhauer illustrates this by an impressive simile: "Abstract knowledge," he writes, "compares with instinctive feeling *as a mosaic compares with a painting* by Van der Werft or Denner; for *however nicely the mosaic may be put together*, the outlines between the stones remain and *no continual transition* from one shade of color to another is possible."[6] Bergson holds the same view, and it is surprising that in another context, which however carries the same meaning, he again uses the very same comparison originally used by Schopenhauer. "A gifted artist paints a picture," he writes.

[4] *Werke*, I, p. 150.

[5] Bergson, "Introduction à la philosophie," *Revue de métaphysique et de morale*, 1903, p. 10.

[6] *Werke*, I, p. 98 f.

"We could *imitate his picture by many colored stones. The more nicely our stones are put together* the more manifold and different they are in color, the better we shall be able *to reproduce their curves and shades. But we should need an infinite number of infinitely small and infinitely differentiated stones* in order to reproduce the picture exactly."[7]

The idea in common, expressed by Schopenhauer and Bergson by exactly the same simile, is that life cannot be perceived by external dissection and recomposition but only by instinctive feeling. This idea is of decisive importance for Bergson's own thought as well as for Schopenhauer's, and our discovery that Bergson and Schopenhauer use the same similes in this context several times indicates that their coincidence is more than a mere curiosity.

* * *

For Schopenhauer as for Bergson life is an object not of understanding but of instinct. In fact, Schopenhauer's philosophy might well be headed "A Study in Instinct" as well as Bergson's. It is therefore quite characteristic for both of these thinkers that they are so greatly concerned with instinct as a biological fact; that both of them pay special attention to insects, because in them instinct is most developed. For Bergson as for Schopenhauer life is not only "the object" of instinct. It would be more just to say that to them instinct is the *living* of life; it is life itself; it is really as it is seen directly, while the knowledge of understanding is neither life nor reality but only an indirect way of preparing reality for practical use.

For Schopenhauer as for Bergson the world of instinct as a world of reality and life stands opposed to the world of understanding as a world of appearance and death. This distinction is of greatest importance for Schopenhauer's philosophy as well as for Bergson's.

So Bergson writes: "If one compares the definitions of metaphysics and of the Absolute he observes that all philosophers in spite of their controversies are at one in distinguishing two absolutely different ways of knowing. The one way stops at the relative; the other penetrates to the Absolute, where it is approachable.... The Absolute is perceived by intuition, everything else by analysis. I call intuition the instinctive sympathy by which we put ourselves into the heart of an object in order to unite with its particular in-

[7] Bergson, *loc. cit.*, p. 2.

expressible essence."[8] With this introduction Bergson plunges the reader into what he considers one of the most central points of his philosophy.

Precisely the same way of "knowing the Absolute" is called by Schopenhauer "the philosophical truth κατ' εξοχην" for his own system,[9] and Bergson and Schopenhauer furthermore agree that this central truth of philosophy, instinctive knowledge as a key for life, is limited at first to the knower's own being. "There is at least one reality," Bergson tells us, "which we all perceive from the inside by intuition....that is we, ourselves."[10]

It is the instinct of our own life of which we are thus aware; but unfortunately this instinct is the only reality of which we dispose directly. We have to transfer our own living instinct to the external world in order to conceive the entire life of the universe. This indirect way of knowledge set forth by Professor Bergson is one of the chief principles of Schopenhauer's philosophy. What Schopenhauer calls "will" is nothing else but what Bergson describes as man's own life immediately experienced by instinctive feeling as the "one reality which we all perceive from the inside," and then transferred to the external world. "Will" is a *determinatio a potiori* for "what we immediately experience as the innermost essence" of our own life, Schopenhauer declares,* and furthermore contends that "to him who knows the most immediate datum of consciousness is will....this conviction will become the key for a knowledge of the inmost essence of all nature. For he will now transfer his immediate experience of life to all those objects which are not given to him in immediate experience."[11]

From this it will be more evident why Schopenhauer's "will" and Bergson's *élan vital* are precisely the same thing. The reason is that both are brought forth by the same "philosophical truth κατ' εξοχην." Both "will" and *élan vital* are philosophical expressions for instinctive feeling conceived as "the living of life" and thence transferred to the external world.

* * *

In this world of "will" or *élan vital* the problem of time again appears. We have already dealt with this problem as far as under-

[8] Bergson, *loc. cit.*, p. 1.

[9] *Werke*, I, p. 154.

[10] *Ibid.*, p. 4.

* *Werke*, I, p. 164.

[11] *Werke*, I, p. 162.

standing is involved in it; we have now to deal with it as far as it extends to the instinctive feeling of life.

In this latter realm the problem takes up the form of an antithesis. Life as instinctively felt is according to Bergson, *durée réelle,* true duration, true time, and to Schopenhauer it is "timeless." Here Bergson and Schopenhauer evidently seem to contradict each other, but as in our former discussion the contradiction is rather in words than in thought. "True duration" means for Bergson time *freed from its spatialization.* "Timelessness" means for Schopenhauer time *freed from past and future.* It means "eternal presence."

It certainly is interesting to compare this conception of Schopenhauer's with Bergson's *durée réelle.* If you ask Bergson why we spatialize time, he would answer that we do so in order to bring past and future to the same level; in order even to conceive of a future and of a past. Hence *durée réelle,* as time freed from its spatialization, is time *deprived of the conception of past and future.* But that is precisely what Schopenhauer calls "*timelessness.*" If consistently carried out, Bergson's *durée réelle* viewed from the point of view of the being himself who exists in *durée réelle* is adequately represented only by Schopenhauer's idea of "timelessness" or "eternal presence." Bergson and Schopenhauer, contradicting each other in their expressions, are logically bound to agree in the fact. Schopenhauer's "timelessness" is *durée réelle,* and Bergson's *durée réelle* ought necessarily be conceived as timelessness.

It is very probable that Professor Bergson himself would contradict this statement. He would point to the obvious fact that again and again he has characterized *durée réelle* as involving both past and future, and hence as something entirely different from Schopenhauer's timelessness. This is very true. But it is no less true that if meant as an objection to Schopenhauer's timelessness Bergson's statement would be very inconsistent.

There are two points of view which should not be confounded. Either *durée réelle* is viewed by an observing outsider or by the being himself who exists in *durée réelle.* An outsider can realize that what this being experiences involves past and future, the being himself however can not. The simple reason for this is that according to Bergson's own doctrine spatialization of time is necessary in order to conceive, however vaguely, of any past or any future. But spatialization of time is not *durée réelle.* Hence a being existing in *durée réelle* does not realize that what he experiences involves

past and future. He does not know either "past" or "future." He lives in timelessness, because he lives in *durée réelle.*

Durée réelle or "timelessness," viewed from the point of view of an outsider, leads towards the conception of evolution. Learning that his own doctrine of evolution was very much like that of Schopenhauer,[12] Professor Bergson thought this coincidence "a happy inconsistency" on the part of Schopenhauer because of the latter's doctrine of "timelessness"; while in truth the apparently missing coincidence in the doctrine of "timelessness" and *durée réelle* was an unhappy inconsistency on the part of Professor Bergson. There can be no thought of evolution from the point of view of the being who himself lives in *durée réelle,* for no evolution can be thought of except in spatialized time. But, in full compliance with the philosophy of Schopenhauer as well as of Bergson himself, an observing outsider may see a very obvious evolution where the being existing in *durée réelle* sees "timelessness" only.

* * *

I return from the problem of timelessness, *durée réelle* and evolution to the main idea of Schopenhauer's and Bergson's thought, to their "philosophical truth *κατ' εξοχην.*" True reality, life, is the instinctive feeling of one's own life-current transferred to the outer world and the world of philosophy as a world of instinct and life is opposed to the dead world of understanding.

This philosophical truth *κατ' εξοχην* is founded on the instinctive feeling of everybody's own life. Hence everybody is a philosopher in so far as he is aware of his own life. He only does not try to express his feelings in conceptual language. Technical philosophy therefore is essentially constituted of everybody's instinctive and intuitive feelings enlarged, systematized and changed into knowledge.

Here again Schopenhauer and Bergson express themselves in a very similar way. So Schopenhauer writes: "By intuition or *in concreto* everybody is conscious of all philosophical truths, but to demonstrate them in abstract concepts and reflective thought is the business of philosophy."[13] Compare with this Bergson's words: "Every lasting system of philosophy is enlivened by intuition at

[12] Compare the very interesting article of Prof. Arthur O. Lovejoy of Johns Hopkins University in *The Monist,* XXI, pp. 216 ff.

[13] *Werke,* I, p. 491.

least in some parts. Dialectic is necessary to demonstrate intuition, to mirror it in concepts and to communicate it to others."[14]

The similarity between Bergson's and Schopenhauer's idea is striking even in its expression. Indeed both philosophies belong to the same well-known type of thought that calls itself "intuitive." Every intuitive philosophy somewhat despises abstract concepts.[15] Still when intuition becomes a philosophy abstract concepts are needed, and they are generously used in the writings of our two thinkers in spite of their contention that nobody enters the Absolute in this way. The gulf between understanding and instinct, theoretically established, is practically bridged by philosophic discourse. There the work of understanding is enlivened by instinct, and the life of instinct is expressed in terms of understanding. None the less instinct and understanding remain theoretically entirely different for both Bergson and Schopenhauer, and it is not their difference but their combination which by the latter is considered as the "riddle of the universe."

This riddle of the universe, and corresponding to it the "philosophical truth *κατ' εξοχην*" of Bergson and Schopenhauer, fairly sums up what I wanted to state in this part of my paper, namely, that these two philosophies are no less similar in the second half of their structure than they were in its first half. A theory of understanding common to both constitutes the first half, a theory of instinctive life as opposed to understanding no less common to both constitutes the second half of their systems. Thus the entire structure of the thought of both is identical in its main outlines.

* * *

It would be possible to show how in some corollaries further instructive similarities between Bergson's and Schopenhauer's philosophy spring up from that main identity.

I shall allude only to Bergson's theory of evolution, which, as Prof. Arthur O. Lovejoy has pointed out in his very interesting paper, exhibits a striking similarity to Schopenhauer's evolutionism.[16] Bergson's doctrine of the unity of life is strangely similar to Schopenhauer's doctrine of the unity of will. For both thinkers this unity forms the basis of their evolutionism. On the other hand it has been pointed out in this paper that Bergson's theory of dis-

[14] Bergson, *Evolution créatrice*, p. 259; "Introduction à la philosophie," *Revue de métaphysique et de morale*, 1903, p. 4.

[15] The only true language of intuition is silence.

[16] See note 12.

section as opposed to the unity of life goes back also to Schopenhauer.

Another point of similarity is to be found in the theory by which Bergson and Schopenhauer explain laughter. Both find its main cause in the sudden perception of a discord between torpid understanding and flexible life.

In their theory of art both lay stress on the artist's power to do away with the pragmatistic narrowness of understanding and to bring man into immediate contact with life itself.

And Schopenhauer's theory of freedom although apparently very different from that of Bergson is likewise founded upon the idea that causality exists only in the realm of understanding and appearance, while life in itself, will, is free and may manifest its freedom to the living being—although the realization of this freedom is conceived very differently by Schopenhauer and Bergson, the former being far more consistent in this respect as in others.

* * *

Professor Bergson is by no means a consistent thinker. On the other hand it is well known that although more consistent than Bergson, Schopenhauer too has justly been accused of great inconsistency. This is one of the chief reasons why in spite of their striking similarity the philosophies of Schopenhauer and Bergson differ widely in more than one respect. Schopenhauer's Kantianism and Neo-Platonism, which are incongruous with the rest of his system, are happily avoided by Bergson. On the other hand Bergson's inconsistencies with regard to his activism and the confusion of his terminology with regard to time and some minor points are avoided by Schopenhauer. That Schopenhauer was more pessimistic than Bergson is of much consequence for the external appearance but of comparatively little consequence for the inner structure of both systems.

Important, however, is the fact that Schopenhauer is far more systematic than Bergson. His philosophy is an attempt to furnish an all-embracing *Weltanschauung* from the point of view of the will to live and its servant, understanding. Bergson's philosophy consists rather of several specific investigations, more or less loosely connected. That is the reason why on the whole Schopenhauer's philosophy is much more imposing and in almost all details much richer than Bergson's thought, while Bergson has the advantage of being less dogmatic and possibly still more stimulating than Schopenhauer.

As to elegance of style, figurative language and happy choice of comparisons no modern philosopher can equal either Schopenhauer or Bergson. They rival each other and both are unsurpassed. However, it seems to me that Professor Bergson's style, especially his figurative language and his predilection for comparisons, is not quite independent of the stilistic habits of Schopenhauer. Our discovery that Bergson uses three similes originally used by Schopenhauer is a strong indication in that direction. Bergson carries a stilistic habit of Schopenhauer to an extreme; but this is only a symptom of a general feature of Bergson's style which is far more onesided and far less varied, but at the same time still more surprising and stimulating, than Schopenhauer's. Thus the differences of thought in the two men are peculiarly mirrored in the differences of their style.

* * *

This leads to the question as to the psychological background for the similarity between Bergson's and Schopenhauer's thought. When Bergson worked out his thought did he plagiarize Schopenhauer? The only answer to this very natural question of a layman in the history of philosophy would be an unmistakable "Certainly not!"

Personally I have good reasons to contend that Professor Bergson is not even aware of most of these similarities, and very likely never was. He himself is probably inclined to think that most of what he actually took from Schopenhauer is his own original thought —original in the popular sense of "creation."

In fact, however, Bergson's thought appears to be "original" not in the popular, but only in the Bergsonian sense of creation. Popular creation is creation out of nothing; Bergsonian creation is the past prolonged into the future. For Bergson all life is creative because it is saturated with the life that has preceded it. Bergson's own philosophy certainly is Bergsonian life *par excellence*. Hence it is creative, because it is saturated with preceding life; and I contend that the preceding life with which it is saturated is Schopenhauer's philosophy.

This is not very difficult to prove, for Bergson himself tells us that he formerly studied Schopenhauer's philosophy closely. Moreover, when Bergson was at the most impressionable and decisive stage of his mental development a Schopenhauer craze was prevalent in France, and *Schopenhaueristes* were seen even in literature and society. Finally, Bergson's teacher Ravaisson was a follower

of Schelling. Indeed some residua of Schelling's philosophy—especially with regard to his theory of matter—are easily discoverable in Bergson's thought. But what is still more important for our investigation, there was no better way to prepare Bergson for Schopenhauer's philosophy than by a knowledge of Schelling, out of whose thought Schopenhauer's own ideas emerged, just as to-day there is perhaps no better way to prepare a mind for Bergson's philosophy than by a knowledge of Schopenhauer, out of whose thought Bergson's ideas have emerged.

* * *

All this leads us back to where we started. The history of philosophy is like one of the ancestral galleries in ancient European castles. Certain family traits reappear again and again in the structure of philosophical systems and Henri Bergson's philosophy bears the family traits of Schopenhauer's "World as Will and Idea."

But Schopenhauer's own philosophy shows family traits as well. It emerged out of the philosophy of Schelling and the general trend of German romantic thought in the early 19th century, and German romanticism again owed a great deal to Herder and Goethe. Indeed I know a passage by Goethe which contains Bergson's entire thought in a nutshell. Using "reason" for "intuition" according to the terminology of his time Goethe says to Eckermann: "The godhead is active in the living, but not in the dead; it works in the growing, the developing, but not in the finished, the torpid. Therefore reason with its tendency for the Divine has to do with the growing, the living; understanding with the finished, the torpid that it may use it for practical purposes."[17] Change Goethe's terminology into the language of Bergson, and the thought expressed by Goethe is almost as Bergsonian as Bergson's own.

This Bergson-Schopenhauer-Schelling-Goethe-Herder type of philosophy could easily be traced back to the old German mysticism and still further back to the ancient philosophy of the Vedanta, with both of which Schopenhauer and most of his German predecessors knew that their own thought was more or less closely connected.

Viewed from its first beginning to its present stage the development of this type of thought goes on exactly in the way which Bergson himself terms *évolution créatrice*. As a living process it enters the thought of a philosopher in the shape last given to it by the preceding generation. As a living process it is itself "creative," i. e., it assumes a new shape different from what it had before; and

[17] *Conversations with Eckermann*, Febr. 13, 1829.

in the brain of the following generation it certainly will change again as a creative power.

Bergson's philosophy proceeds from an *élan vital* of thought. This explains why it is saturated with the past and, as we may hope, pregnant with the future. The past with which it is saturated, however, is neither pragmatism nor any American nor English philosophy, for all these mean typical work of "understanding," while for Bergson philosophy begins where understanding ceases. The *élan vital* in Bergson's own philosophy is German and characteristic of the close affinity between German and French philosophy—an affinity which may be traced back all through the history of human thought. In former ages the influence of French thought on Germany preponderated over the influence of German thought on France. Since the beginning of the nineteenth century it is the influence of German thought on France which has preponderated because there was a feeling that the *élan vital* of German thought is creative and pregnant with a future. Never was its creative power developed with more splendor and force than in Henri Bergson's philosophy.

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HENRI POINCARE: OBITUARY.

On July 17, 1912, the world lost the great French mathematician whom Karl Weierstrass—one of the greatest mathematicians of the nineteenth century—when writing to Sophie Kowalevsky, specially singled out as one of the most eminent of the school of younger French mathematicians.[1]

Jules Henri Poincaré was born at Nancy on April 29, 1854. He came of a family of which various members have risen to eminence. His father was professor in the Faculty of Medicine at Nancy, and wrote, among other works, on neurology at a time at which such researches were only pursued by a small number of scientific men. An uncle, Antoni Poincaré, wrote on meterology; and, of his two sons, one is M. Raymond Poincaré, the present President of the Ministerial Council, and the other is M. Lucien Poincaré, who is Director of Secondary Education and Minister of Public Instruction. Henri Poincaré's only sister married M. Emile

[1] Cf. G. Mittag-Leffler, *Compte rendu du deuxième congrès international des mathématiciens tenu à Paris....1900*, Paris, 1902, pp. 145-148.

Boutroux, the distinguished philosopher, and their son, M. Pierre Boutroux, is a well-known mathematician.

Henri Poincaré was precocious, intellectually, and entered the Ecole Polytechnique in 1873, and in 1875 the School of Mines as engineering pupil; in 1879 he gained the degree of Doctor of Mathematical Sciences at the University of Paris; in the same year he joined the Service of Mines as engineer; in 1881 he became professor at the Faculty of Sciences in Paris; in 1887 he was elected a member of the Academy of Sciences; and in 1908 he was elected one of the forty "immortals" of the French Academy.

A biography of Poincaré and a bibliography of his works has been published by Ernest Lebon.[2] Poincaré's first original researches were in pure mathematics. In 1880 the Academy of Sciences proposed the theory of differential equations as the subject of the great prize. Poincaré sent in a sketchy memoir with the title "Non inultus premor"—that of the town of Nancy—which did not gain the prize but which Charles Hermite mentioned encouragingly in his report. From the beginning of 1881 the subject—the integration of certain linear differential equations—was developed with surprising genius and rapidity in a series of papers presented weekly to the Academy of Sciences. Weierstrass, who admired these papers so warmly, thought it a pity that Frenchmen published their discoveries in a succession of little papers. But surely the psychological interest is heightened by this mode of publication. We know that Poincaré worked almost subconsciously, and often had no idea of what he was going to discover. Gauss's motto was, *Pauca sed matura,* and even now almost every publication of his is an almost perfect and complete classic; and yet how greatly do we feel the need of some indication as to how these discoveries grew. Weierstrass reminds us in many respects of Gauss. His works, too, were never quickly published, and very many important things he found or views he held were either not published at all, or only long after he announced them, and then by his pupils. The case is different with Poincaré. One of the many reasons for which he will live is because he has made it possible for us to understand him as well as to admire him.

Poincaré's name is associated, for the pure mathematician, with the "Fuchsian," "Thetafuchsian," and "Zetafuchsian" functions. We now call them, after Felix Klein, "automorphic" functions. But we

[2] *Henri Poincaré: biographie, bibliographie analytique des écrits*; 2d ed., Paris, Gauthier-Villars, 1912 (collection "*Savants du Jour*").

can only refer to his other researches on the theory of functions and his allied work on the theory of numbers, and will now turn to his works on astronomy and physics.

Poincaré's investigations on the form taken by a gravitating mass of fluid in rotation (1885-1901) led him to interesting theories on the parting of the earth and moon and the formation of variable stars. His researches on the stability of the solar system, which consisted in the revision of Laplace's calculations and the carrying of them to a higher order of approximation, showed that Laplace's theory of 1784 was quite just. These and other results are contained in Poincaré's three volumes on the new methods of celestial mechanics.[3] Here we must also refer to his works on the tides and on the problem of three bodies. On mathematical physics, Poincaré published many volumes of lectures given at the University of Paris and elsewhere on light, electricity—including the theory of Maxwell—capillarity, vortices, potential, thermodynamics, the theory of the conduction of heat, elasticity, and the theory of wireless telegraphy. Besides these, his lectures on the calculus of probabilities and on various subjects in celestial mechanics have been published.

Some very interesting psychological and physical details about Poincaré were published in 1900 by Dr. Toulouse as the second volume—the first was chiefly occupied by a study of Emile Zola—of his *Enquête medico-psychologique sur la supériorité intellectuelle.*[4] The help given to the scientific answering of the question: "Le génie est-il une névrose?" by such studies is, of course, immense; but most of my readers are more concerned with the qualities associated with the great mathematical capacities of a man who took such a keen interest in questions on the border-line between mathematics and philosophy.[5] It is impossible to read Dr. Toulouse's book without gaining a very vivid picture of the personality of Henri Poincaré. It is always deeply interesting to read authentic accounts of the methods of work of mathematicians, and for some years past, MM. H. Fehr, Th. Flournoy and E. Claparède have conducted an inquiry on this subject in the columns of *L'Enseignement mathématique.* Poincaré himself, in a well-known article published in 1908, has made some striking observations on his own process of mathematical discovery. And we must, I think, bear in mind, when

[3] *Les Méthodes nouvelles de la Mécanique céleste,* Paris, 1892-1899.

[4] *Henri Poincaré*; Paris, Ernest Flammarion.

[5] Poincaré's work in this direction is well known to readers of *The Monist* by the translations of George Bruce Halsted.

reading Poincaré's articles on the logic of mathematics, that they are the work of a man who was primarily—perhaps almost exclusively—interested in the faculties of invention. When mathematical logicians asserted that the whole of mathematics follows by logical principles alone from concepts which can be logically defined and from the primitive propositions of logic alone, Poincaré and many other mathematicians objected that "intuition" was left out of account. There is a great likelihood that this is not really Kantianism in mathematics; only phrases make it seem so. Kant clearly recognized the distinction between the question as to whether a truth B is logically implied by a truth A and that as to whether B is discovered by a certain person who starts from the premise A alone and uses only purely logical considerations. The mathematical logicians do not deny to the seeker of truth either genius or the creative power —if such exist—of the artist; they are concerned with an epistemological question, and psychological objections are irrelevant there. The case is analogous to this: If someone were to point out that the properties of logarithms are simple consequences of the conception of one number as a power of another, he would not be confuted by the remark that Napier did not invent logarithms in that way; or again, it is not relevant to the student of Keats's poetry, as such, to know what porridge John Keats ate.

If this interpretation of the attitude of the "creative" mathematicians is correct, their position with respect to mathematical logic is easily explained. That the interpretation is correct seems supported by Poincaré's last controversial work on mathematical logic which he gave this year as a lecture to London University, and which has just been printed in *Scientia*.[6] In the previous discussions on the use of the infinite in mathematics, in which Poincaré joined, each side kept on repeating the same arguments. There seems, in fact, a fundamental difference in mentality among mathematicians. Some, whom Poincaré called "pragmatists," believe that the infinite is derived from the finite, and all verification and all definition is performed with a finite number of words; others, the "Cantorians," believe that there are objects and truths which cannot be defined or demonstrated in a finite number of words. The Cantorians are realists and believe that the truth of a proposition does not depend on its verification by us. It is not difficult to place Poincaré, on the

[6] "La Logique de l'infini," *Scientia* (*Rivista di Scienza*), July, 1912, pp. 1-11.

grounds of some of his writings, among those whom he not inappropriately calls "pragmatists."

When Poincaré was five years old, he had a severe attack of diphtheria, and partial paralysis. All this made him rather weak for a long time, and perhaps was the origin of his lifelong clumsiness. Of his absence of mind, many stories are told. Once during a walk, he was suddenly surprised to find a wicker bird-cage in his hand. He had unconsciously removed it from a wayside stall.

As regards religion, at the moment of his first communion he was a believer; then belief left him gradually, and, from the age of eighteen he was a sceptic. In politics he was a republican; he held to the principle of personal property; he believed in political equality and the political rights of women,—but here he feared clerical influence.

In mathematics, he cannot be said to belong to any school. In a short life not without physical drawbacks, he has, by regular work, produced about 500 writings—some of them of the very first order.

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HENRI POINCARE: AN APPRECIATION.

The foremost of Frenchmen is no more. When Laplace was asked to name the greatest German mathematician he answered, "Pfaff." "But how about Gauss?" said the inquirer. "Ah," replied Laplace, "he is the greatest of all mathematicians." Similarly we might modify our first statement and declare that the foremost of all men is no more. For on July 17, having apparently recovered almost completely from a surgical operation undergone only a few days before, Henri Poincaré, while dressing himself in the morning, was suddenly smitten with an embolism and fell dying in the arms of his wife. While the delegates of learning from all quarters of the globe were assembling in London to celebrate the 250th anniversary of the Royal Society, instantly the brightest star in the galaxy of the sciences was eclipsed forever. The sad intelligence was at once flashed around the world, but the details as set forth in the Paris journals of the 18th are but lately at hand.

Commanding the homage and admiration of all, so generous, so pure-hearted, so noble-minded was Poincaré that he aroused the envy and jealousy of none. If "Freedom shrieked when Kosciusko fell," with far more propriety may universal Science, may Philosophy

herself, weep at the passing of her illustrious son. For Poincaré was not a mere specialist, an isolated summit of technical learning, but rather a mountain range of knowledge. His Andean intellect traversed the whole continent of science. In physics, in mathematics, in astronomy, in logic, in philosophy even, he strode from peak to peak in the heights of thought, and wherever his feet touched, there was a blaze enkindled. His compatriots say that France has borne no equal in a hundred years, not since d'Alembert and Laplace. To a foreigner it may be questionable whether the limit may not be pushed much further back, even to the days of Descartes. For while the mind of Poincaré did indeed cast off no single orb of thought to match at once in largeness and in luster the *Mécanique céleste,* or still more the *Théorie analytique des probabilités,* yet it has studded the firmament of exact science with a host of splendors. Scarcely if at all inferior to Laplace or even to Lagrange as analyst, as geometer, as physicist, as astronomer, Poincaré was what they were not —he was a logician of the first order and a philosopher, profound, penetrating, and spiritual. Nor was this all; for his genius in exposition allied him with Clifford and brought him into livelier sympathy with the lay intellect than almost any of his peers in the realm of pure science, while his fine artistic nature and literary sense expressed themselves in a style at once clear and concise, nervous, vivid, picturesque and animated. As subtle as Hume, as comprehensive as Helmholtz, he was least of all a dry-as-dust savant; the keenest of logicians, he did not disdain the graces of rhetoric, but poured out for his fellows the divine draughts of his thought in golden goblets of speech.

It was not, however, as mathematical physicist, as analyst of Fuchsian functions, not as student of the stability of the solar system, not as discoverer of unsuspected figures of equilibrium, not as master of metageometry, not as preeminent logician of science, not as any nor as all of these, that Poincaré rendered his highest service to humanity. It is his supreme merit to have recognized explicitly the inalienable rights of the human spirit, to have opposed firm as Gibraltar the rising tide of naturalism and the pride of knowledge which, intoxicated with the triumphs of physical science and its applications, refuses to see any mystery beyond sense remaining in the world, and boldly aspires by means of mind to pull down mind from its throne and to reduce the universe to a molecular maelstrom, to a wisp of granulated ether. When Napoleon asked Laplace about having written so great a book without once naming the name of God

therein the savant replied, "Sire, I have no need of that hypothesis." Lagrange, a finer spirit, on hearing of this commented: "But it is a beautiful hypothesis that explains many things." In his famous *mot* Laplace has declared his kingship and at the same time defined his kingdom. True, in the realm of matter he had no need of the beautiful hypothesis; in the kingdom of causality, of mass and motion, there is no purpose, no reason, and hence no need of God, the Reason of all. But Poincaré saw through all the phantasies of "scientists," as the astronomer sees through the nebula in Orion, and beheld far behind the phenomena of time and space the eternal realities of self and of soul; while peering into the processes of physical nature deeper even than Laplace himself, he never forgot that they are after all an unsubstantial pageant, that

> "On earth is nothing great but man,
> In man is nothing great but mind."

So he became in a sense the moderator of the assembly of the sciences. As no other living man he could say, "Thus far and no further;" for he spake as one having authority. Even the Germans, who are seldom over-quick to acknowledge the hegemony of others in the ranks of thought, forgot all national and racial prejudices in the presence of Poincaré and freely declared him to be "the first authority of this age" (*die erste Autorität von dieser Zeit*).

The savant closed his eyes at the age of fifty-eight in the full flush of his powers, at the fever-heat of his intellectual activity. What more he might have done, who knows? But assuredly his mantle will fall upon worthy if not upon equal shoulders; in the paths he has broken there will follow increasing throngs. It is our human form of speech to deplore an irreparable loss. But in some larger, deeper and higher, though indefinable, sense there is perhaps only gain forevermore.

> "One accent of the Holy Ghost
> The heedless world hath never lost."

Le roi est mort: *vive le roi.* Poincaré is dead: but deathless is Poincaré.

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THE CAPTURE THEORY OF COSMICAL EVOLUTION CONFIRMED BY THE LATEST RESEARCHES ON THE ORIGIN OF STAR CLUSTERS.

Introductory Remarks.

It is a very remarkable fact that since the epoch of Sir William Herschel little serious consideration has been given to the origin of star clusters, until within the last few years. Accordingly the thoughtful suggestions thrown out by that unrivaled man long proved largely if not entirely barren of fruitful results for the development of a science of cosmogony, because his early ideas were lost sight of or forgotten. We have labored under a strange delusion, of preferring the theories of Laplace to those of Herschel, but have at last found the way from darkness to light, from error to truth. And it happens that of all the investigations yet made in cosmogony those relating to the neglected subject of star clusters are the most convincing and least open to objection.

In a paper entitled "Dynamical Theory of the Globular Clusters and of the Clustering Power Inferred by Herschel from the Observed Figures of Sidereal Systems of High Order," recently communicated to the American Philosophical Society held at Philadelphia, and just published in the *Proceedings* of that illustrious society, I have examined the whole problem of the origin of clusters in a somewhat exhaustive manner. By the use of mathematical methods of rigorous character, I was able to develop the most convincing proofs that these aggregations of stars have arisen by the process of capture in the course of millions of ages. It will be the main purpose of this article to discuss the results arrived at in this general investigation of the origin of star clusters; but before taking up this subject in detail it will be allowable to treat briefly of the conclusions reached by the illustrious Poincaré in his *Leçons sur les Hypothèses Cosmogoniques,* 1911, and to notice also the unimportant objections advanced by Prof. Charles André in *Scientia,* No. 2, 1912.

The Views of Poincaré.

In his *Leçons sur les Hypothèses Cosmogoniques,* 1911, M. Poincaré gives a summary of the various theories of cosmogony, and in two chapters discusses results arrived at in my *Researches on the Evolution of the Stellar Systems,* Vol. II, 1910.* He exam-

* These chapters appeared in translation in *The Monist* of July, 1912.

ines and adopts the proof therein given that the roundness of the orbits of the planets and satellites is due to the secular effects of the action of a resisting medium. He concurs in the capture of satellites, as well as in the new theory of spiral and ring nebulae. M. Poincaré especially remarks how well the resisting medium explains the roundness of the orbits of the planets and satellites; and altogether is favorable to the recent development of cosmogony into a new science of the starry heavens.

One point in my work, however, has been slightly misunderstood by Poincaré, and I will therefore dwell upon it here. After outlining the leading principles of the Capture Theory he adds that while it fully explains the roundness of the orbits, it does not give a satisfactory explanation of the inclinations of the planetary orbits. But here he has evidently lost sight of the nature of the spiral nebula from which our solar system is supposed to have arisen.

The Capture Theory means primarily that all revolving bodies are added on from without,—the planets being added to the sun and the satellites added to the several planets; but it is not held that the entrance into our system was from all directions over the celestial sphere and thus entirely at random. On the contrary it is carefully explained in my work that the system from the beginning had a fundamental plane of maximum areas, due to the fact that a spiral nebula is formed by two principal streams coiling about one another; and the plane of maximum areas is the plane determined by these predominant streams.

In the condensation of a nebula an infinite number of minor streams probably are involved; but the whirling motion is made possible only by two predominant streams, as shown in photographs of spiral nebulae. That is to say, there is more matter in the two large streams than in the smaller ones; and this gives a fundamental plane to the system when it becomes mature, just like that in which the planets of the solar system are found to move. The planetary orbits ought not to lie exactly in the same plane, but near an invariable plane, such as Laplace in 1784 proved to exist in every system of bodies subjected to the mutual gravitation of its parts.

The comets or smaller masses of nebulosity naturally should be inclined at all angles to the invariable plane; but as they intersect that plane twice in their orbital motion about the sun, they will sooner or later pass near a planet revolving in an orbit lying near the fundamental plane of the system, and their orbits are thus subjected to profound changes of position as well as of form and extent. Count-

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less comets are destroyed in building up the planets; so that much matter not originally lying in the plane of the planets is finally captured and drawn into that plane, as the masses of the planets gradually augment, and they are drawn nearer the sun in orbits becoming ever smaller and smaller, and rounder and rounder.

Thus on the one hand the resistance of cometary matter reduces the size of the planetary orbits and makes them rounder, while on the other hand the growth of these masses increases their mutual attraction; and if they were originally near the plane of the predominant streams, in time they come to move almost exactly in one plane, as now observed in our actual planetary system.

For as our system has shrunk, and the original orbits were hundreds of times larger than at present, the nuclei at the outset were not necessarily very near the plane in which they now move, but may have departed from it considerably. Mutual inclinations of a few degrees now found in our system are magnified at hundredfold primordial distances into very great absolute distances; so that the original streams need not have been at all compressed, but may have been exceedingly diffuse, just as actual nebulae appear to be.

Accordingly it is a remarkable fact that the theory which accounts for the roundness of the orbits of the planets also explains the small mutual inclinations of their orbits, and the rotation of the sun about an axis nearly perpendicular to the plane in which the planets revolve. The explanation of the origin of our system from a spiral nebula thus appears to be entirely satisfactory.

The Views and Objections of André.

The objections to the Capture Theory advanced by André are easily shown to be without the slightest foundation. It is quite unnecessary to consider most of them, and I will therefore content myself with the three chief ones, which will sufficiently show the weakness of the rest.

1. André claims that the spherical expansion in Babinet's criterion as I have used it is not strictly in accordance with Laplace's theory, because Laplace did not imagine the sun's atmosphere to be expanded in a spherical form, but rather in the form of a flat disc. This objection is quite devoid of foundation, as will appear from the following simple considerations.

a. If the expansion be spheroidal, as a flat disc, more of the matter is at greater distance from the center, for given volume, than in a spherical expansion; so that the moment of inertia is in-

creased, and with constant moment of momentum the angular velocity is therefore *decreased*. Hence a discoidal expansion of the sun is more unfavorable to Laplace's hypothesis than the spherical expansion used by me. For in case of a sphere the moment of inertia is shown in works on the calculus to be $2/5(Mr^2)$, where r is the radius and M the mass; in an ellipsoid with equatorial axes a and b it is $M/5(a^2+b^2)$, and when $a=b$, as in an ellipsoid of revolution, this becomes $2/5(Ma^2)$, a being the equatorial axis.

b. To reduce this to numbers I took ellipsoids with meridian sections of eccentricity 0.10, 0.25, 0.5, and 0.8, giving oblatenesses of 0.00501, 0.03176, 0.13397, and 0.40000 respectively; and found $a^2=1.00336r^2$; $a^2=1.0217r^2$; $a^2=1.1525r^2$; $a^2=1.4057r^2$. *This shows how the moment of inertia increases as the oblateness increases, and thus proves a corresponding decrease of the angular velocity of rotation below that published in my tables of Babinet's criterion.* The objection of André therefore has not the slightest foundation, because my calculations are more favorable to Laplace's theory than those based on the theory of an oblate spheroid.

2. André dwells on the fact that Laplace imagined only *the atmosphere* of the sun expanded to the orbits of the planets. But as the sun itself when so expanded becomes much rarer than most atmospheres we are familiar with, it is readily seen that this point is not well taken. When the sun is expanded to Neptune's orbit, the average density of the nebula is 260 million times less than that of air at sea level. Nothing more need be said on this point. Such a medium could exert little or no hydrostatic pressure from the center, and Laplace's theory of the detachment of zones of vapor under conditions of hydrostatic pressure implies that he overlooked the rarity of this medium, which makes such a thing as hydrostatic pressure quite impossible. No alteration of central arrangement of density would materially change this result, and we may thus dismiss it without further comment.

3. As the centrifugal force, by Babinet's criterion, is only a ten millionth part of that required to detach the earth, and a three hundred millionth of that required to detach Neptune, while the hydrostatic pressure likewise is insensible, it is clear that no such detachment as Laplace imagined ever took place. André, Ligondes and other French writers are simply injuring the memory of Laplace by presenting to the Paris Academy of Sciences conclusions which would be immediately rejected by Laplace himself if he were living to-day.

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After having studied the works of this great master of celestial mechanics from the days of my youth, I believe I have followed his spirit in rejecting what is now known to be false. Professor André is in the unfortunate position of having written books favorable to the abandoned theory of Laplace; but he should aim at truth rather than perpetual consistency, and modify his views to meet the latest discoveries in science. For a true philosopher does not aim at supporting his earlier writings, but at gradually attaining the truth, even if his first work has to be modified or entirely abandoned. The successors of Laplace obviously should act upon this laudable principle.

4. Even if the retrograde satellites and a multitude of other phenomena did not tell us unmistakably that all the satellites have been captured, and we still tried to explain these bodies by the detachment theory of Laplace, we should remain quite in the dark as to the origin of the observed rotations. They would be simply assumed, and not explained; and so we should have no rational theory of the formation of the solar system; whereas the Capture Theory gives a simple and natural explanation of the rotations and obliquities as well as the orbital motion of the satellites, and the variations of their brightness, the lunar craters and maria and kindred phenomena; and all the phenomena are so woven together that it is impossible to doubt the truth of the new theory.

In the same way, even if the solar nebula could have rotated rapidly enough to detach zones of vapor as Laplace imagined, it would still be impossible to account for so rapid a rotation. Fortunately Babinet's criterion shows that no such rapid rotation for the detachment of zones of vapor ever took place; and that Laplace was deceived by the roundness of the planetary orbits, which we now recognize to be due to the secular action of the nebular resisting medium formerly pervading our solar system.

Necessity for Wider View of all Sidereal Systems.

It requires no elaborate argument to convince any philosophic investigator that the laws of cosmical evolution can best be deduced from the study of nature in the widest sense. The narrowness of the cosmogony of Laplace arose from the fact that it was based wholly on our solar system, and that too before the system was fully understood. The roundness of the orbits of the planets and satellites and the survival of a ring about Saturn led to the idea

that all these bodies had originated by the detachment of rings. Yet as soon as the orbits of the double stars were determined, they were found to have eccentricities of every degree, between the round orbits characteristic of the planets and satellites and the very elongated orbits characteristic of the comets. The development of double stars obviously could not have been by the formation of rings as imagined by Laplace.

Accordingly without such a comprehensive view of the different types of systems it would be vain to hope for the deduction of a general law of nature. The folly of adhering to the old methods of Laplace based on an imperfect knowledge of the solar system alone is thus apparent; and after what is now shown, from Babinet's criterion, as to the impossibility of detaching masses or rings, there is no course open to us but to reject Laplace's hypothesis once for all. It does not give us a general law of nature, and is not true even for the special case of the solar system.

Our hope for finding the law of nature must be based on the study of double and multiple stars, and sidereal systems of higher order. Now it happens that of the various sidereal systems known to the astronomer, the globular clusters are the most complex, and at the same time the most symmetrical and regular in their constitution. If therefore any light can be obtained on the formation of sidereal systems of such high order, it might be possible to derive principles which could be applied to less symmetrical systems of lower order. This is what I have done in my recent investigation of the origin of clusters. Having deduced the law of nature from the highest and most complex systems, with wonderful regularity of figure, I have proceeded to apply it also to systems of the lowest type, as the solar system and the double and multiple stars. This new method of procedure is so important, that it becomes advisable to explain it in some detail.

Nature of Clusters, Average Distance of the Stars Apart, Increase of Density Towards Center.

Sir William Herschel always considered the globular clusters to be the most wonderful of all sidereal systems. He never ceased to marvel at the existence of these swarms of stars, which were known to be aggregations of suns; and he inferred that at length they had been moulded into the spherical form by the action of central powers.

Even in the time of Herschel it was recognized that the clusters are very far from the earth, and thus that the component stars are not really close together, but separated by intervals which are very great compared to those which separate the planets from the sun.

More modern discovery has confirmed the sagacious conjectures of the great Herschel. The latest investigation of the profundity of the Milky Way, which I finished in November, 1911, and have just published in the *Proceedings* of the American Philosophical Society at Philadelphia, shows that the remotest clusters are removed from us by at least a million light-years. Indeed this determination of the depth of the Milky Way shows that the remotest stars may be removed from us by distances of five or ten million light-years; but even with most of the clusters at distances of hundreds of thousands of light-years, it is possible to say with certainty that the average space between the stars in globular clusters is of the order of a light-year, which is 63275 times the distance of the earth from the sun. We thus have the spectacle of systems of stars separated by great intervals, but so remote as to be drawn together by perspective into a small angular space on the surface of the sky.

The density in these masses of stars was found by Herschel to be always greatest towards the center; and in fact to be in excess of that corresponding to the supposition of equal scattering. Herschel therefore inferred that the accumulation in the centers of the clusters must be due to the secular action of a clustering power, which he believed to be nothing else than universal gravitation working over millions of ages. He remarked that the Milky Way presented the aspect of a clustering stream traversing the heavens as an irregular band of milky light; and as he had found the sidereal universe to be greatly extended in the direction of the plane of the Milky Way, he correctly inferred that the clustering stream thus presented to the eye was the effect of distance and of local aggregations of the stars into star-clouds and clusters. The stars are spread out into a comparatively thin stratum, and at great distance the effect is to give the appearance of the Milky Way, which thus appears as a clustering stream several degrees in width.

How the Stars are Captured in Clusters.

In the memoir above referred to I have established the capture of stars by a cluster, and the secular shrinkage of the cluster, by the use of Green's theorem for the transformation of a triple in-

tegral appropriate for space into a double integral over the surface of the cluster. By showing that the surface shrinks as the result of close appulses among the stars, and also as the outcome of mutual gravitation, even when no close approach occurs, it is found that the cluster becomes more and more compressed, with density accumulating towards the center.

The attraction of members of a cluster is analogous to surface tension in working to decrease the volume of a bubble, or in rounding up a drop of dew, to give minimal surface for a given volume. In the same way gravity tends to make a planet perfectly round, except as modified by rotation into an oblate figure. Herschel used such analogies in his argument for a clustering power, which he inferred to be moulding the figures of clusters. And recently I have tested his suggestion mathematically, and found a conclusive proof that the argument is correct.

To give a simple analogy for the capture of stars in clusters, with known processes in the solar system, we may remark that Jupiter captures the comets crossing over his orbit, and transforms their paths till they lie wholly within that of the planet. In this way he has captured quite a family of comets and thrown their orbits within his own orbit. Now in the memoir above referred to I have shown that a *shell of stars* in a cluster acts very much as Jupiter does on the comets—and thus tends to reduce the path of an oscillating star till it comes within the confines of the shell.

Accordingly if a star from without once enters a cluster, and thus begins to traverse the series of shells of which the cluster is made up, it will never quit the swarm but be gradually drawn in, and captured, during one or more complete oscillations. The extent of its outward journey from the cluster, if any occurs, will be decreased, until finally it is dragged down to the level of the shell, and becomes a member of the cluster. This is one of the most remarkable results of our dynamical theory of clusters. The Capture Theory being thus verified for these globular masses of stars, it naturally may be expected to operate in systems of lower order.

No Possible Origin of Clusters Except that Outlined by the Capture Theory.

The globular clusters are so perfectly symmetrical that they become of high interest in elucidating the problems of cosmogony. For it is not conceivable that systems of such large mass, great

extent and perfect symmetry, can have arisen, except by the gathering together of stars from a wider extent of space.

No process of collision, for example, would account for the globular clusters; for by impact the matter of two hypothetically disrupted masses would neither be symmetrically distributed nor dispersed over such a vast space as that now occupied by the thousands of suns composing a cluster. Then, again, to be effective such hypothetical collision would have to be between approximately equal giant suns; and there are too few stars of such enormous mass for pairs of them ever to come into bodily collision.

Accordingly, a little consideration shows us, on the one hand, that such giant collisions would not occur; and, on the other, that even if they could take place such widely diffused and symmetrical swarms of stars could not arise by this process. The globular clusters therefore are due to the aggregation of stars once symmetrically and widely distributed in space. This gives us a good illustration of the Capture Theory on the most stupendous scale. Similar views were reached by Herschel, without mathematical investigation of the subject, such as I have recently developed; and it may be remarked that he found the evidence of a clustering power most convincing.

The *New General Catalogue of Nebulae and Clusters,* published by the Royal Astronomical Society of London in 1888, contains a list of more than one hundred globular clusters, mostly distributed along the course of the Milky Way. The clustering of the stars into great systems about so many centers shows how general and widespread this tendency is in nature.

If now we recall that only the oldest sidereal systems can have attained a state of perfect symmetry, it is obvious that a larger number of sidereal systems might be expected to have an irregular and unsymmetrical aspect. The globular clusters are therefore only a part of the aggregations of stars exhibiting the effect of the clustering power; but the perfection of this type of system renders it eminently adapted to disclosing the process by which all clusters are formed. For if the law of nature can be deduced from the perfect type of sidereal development, it may with equal certainty be inferred to operate in those sidereal systems which have not yet attained to full maturity. By investigating the different types of sidereal systems our studies may thus disclose the general law of cosmical evolution and embrace phenomena extending over millions of ages!

The Law of Nature Embraces also Systems of Lower Order, and Therefore the Planetary System and the Systems of Double and Multiple Stars.

Those who believe in the uniformity and continuity of the laws of nature, as laid down by Newton in the *Principia,* 1687, will quickly realize that the law of cosmical evolution established for the globular clusters should necessarily hold also for systems of lower order. Rule I: "We are to admit no more causes of natural things than such as are both true and sufficient to explain their appearances." Rule II: "Therefore to the same natural effects we must, as far as possible, assign the same causes."

Accordingly, in line with these rules of Newton, I have shown that the Capture Theory will explain the formation of the solar system, as well as the double and multiple stars: and having found the principle to be the same throughout the sidereal universe, I have inferred that nature's law everywhere is one of adding on from without. The component stars are added to the clusters, and drawn nearer and nearer the center; the planets added to the sun and made to revolve in smaller and smaller and rounder and rounder orbits. Likewise the satellites were added on to their several planets, and the moon captured by the earth. The double and multiple stars were formed on the same principle—the nuclei having originated in the distance, and subsequently approached the centers about which they now revolve. This gives us a general law of nature of the utmost simplicity.

And not only is the generality of the law proved by force of analogy, but also by direct mathematical demonstrations in the solar system, deduced from Babinet's Criterion; while in the clusters the proof is so obvious that it need scarcely be emphasized. The demonstration of this law in the double and multiple stars is similar to that available in the solar system; and moreover is supported by the analogy of the clusters, into which the multiple stars merge by insensible degrees, when the number of bodies in a group is increased indefinitely.

Accordingly nothing is more certain than that the law of cosmical evolution now recognized is the true law of nature. It does not even resemble the abandoned theory of Laplace, but has considerable resemblance to the general outline of the nebular hypothesis as traced by Sir William Herschel. In particular Herschel's theory of clusters, as originating by the aggregation of isolated

stars is deserving of attention; for this is the earliest outline of a process of capture similar to that now worked out in detail and shown to be applicable to all types of systems observed in the sidereal universe.

In conclusion it seems advisable to close this discussion by the following summary quoted from my latest memoir on *The Dynamical Theory of Clusters.*

Summary and Conclusions.

Without attempting, in this closing section, to recapitulate the contents of this memoir in detail, it may yet be well to draw attention to some of the most significant conclusions at which we have arrived.

1. As intimated in the first section of this paper the problem of n-bodies, under ideal dynamical conditions, remains forever beyond the power of the most general methods of analysis; but the dynamical theory of clusters gives us the one secular solution of this problem found under actual conditions in nature. For when n is of the order of 1000, so as to give rise to a cluster, the clustering power observed by Herschel operates to exhaust the mutual potential energy of the system, and bring about increasing accumulation in the center, so that the cluster finally unites into a single mass of enormous magnitude. Probably the giant stars of the type of Canopus and Arcturus have arisen in this way.

2. And since attendant bodies of every class—as satellites, planets, comets, double and multiple stars—tend everywhere to approach the centers about which they revolve, as an inevitable effect of the growth of the central masses and of the action of the resisting medium over long ages, it follows that the secular solution of the problem of clusters is more or less valid for all cosmical systems. They finally end by the absorption of the attendant bodies in the central masses which now govern their motions.

3. The dynamical theory of globular clusters shows that the clustering power inferred by Herschel is nothing else than the action of universal gravitation; and that it operates on all sidereal systems, but does not produce the cumulative effect which Herschel ascribed to the ravages of time inside of millions of ages.

4. The globular clusters are formed by the gathering together of stars and elements of nebulosity from all directions in space; and this points to the expulsion of dust from the stars of the Milky Way,

and its collection about the region of the formation in such manner as to give essential symmetry in the final arrangement of the cluster, which doubtless has some motion of rotation, and originally a tendency to spiral movement.

5. The stars and smaller masses are captured by the mutual action of the other members of the cluster, and worked down towards the center of the mass. This gives a central density in excess of that appropriate to a sphere of monatomic gas in convective equilibrium (A. N. 4053 and A. N. 4104).

6. The density of the clusters is greater on the outer border than in a globe of monatomic gases, which shows that stars are still collecting from the surrounding regions of space. The starless aspect of the remoter regions about clusters is an effect of the ravages of time, as correctly inferred by Herschel in the course of his penetrating sweeps of the starry heavens.

7. And just as clusters under the mutual gravitation of the component stars contract their dimensions, with time, chiefly owing to the growth of the central masses, so also do other systems, whether the mass-distribution be *single,* giving a system made up of a sun and planets, or *double, triple* and *multiple,* giving binary, triple or multiple stars, or sidereal systems of still higher order. The tendency everywhere is from a wider to a narrower distribution of the large bodies; while the only throwing off that ever occurs is of particles driven away from the stars by the action of repulsive forces.

8. The orbits of the stellar and planetary systems are decreased by the growth of the central masses and rounded up by the action of the nebular resisting medium. And in like manner all clusters tend to assume spherical or globular figures, so as to justify the expression of Plato, that the Deity always geometrizes; or Newton's remark that the agency operating in the construction of the solar system was "very well skilled in mechanics and geometry."

9. Newton required the intervention of the Deity to give the planets revolving motion in their orbits, because in the absence of repulsive forces he could not account for the dispersion of the matter so as to produce the tangential motions actually observed. By means of the theory of repulsive forces, however, it is now possible to explain these projectile motions, which Herschel likewise pointed to as the chief agency for the preservation of sidereal systems. The only assumption necessary is an unsymmetrical figure of the primordial nebula, giving a whirling motion about the center as the system develops; and since the dust gathers from all directions it is certain

that this lack of perfect symmetry will always develop, as we see also by the spiral nebulae.

10. It is this unsymmetrical form of the spiral nebulae produced by the gathering of the dust from the stars, or the slight relative tangential motion of stars formed separately but finally made to revolve together as a binary system, that gives the projectile forces with which they are set revolving in their orbits. In no case have they resulted from the rupture of a rotating mass of fluid under conditions of hydrostatic pressure as formerly believed by Darwin, Poincaré and See.

11. Even if the rotation could become rapid enough to produce a separation, under conditions of hydrostatic pressure, by rupture of a figure of equilibrium, there would still be the equal or greater difficulty of explaining the origin of the primitive rapid rotation. This last difficulty escaped notice till we came to assign the cause of rotations, and found that mechanical throwing off was impossible under actual conditions in nature. It is therefore recognized, from the definite proof furnished by Babinet's criterion in the solar system, that such a thing as a throwing off never takes place; but that all planetary and stellar bodies are formed in the distance, and afterwards near the centers about which they subsequently revolve.

12. This gives us a fundamental law of the firmament—the planets being added on to the sun, the satellites added on to their planets, the moon added on to the earth, and the companions added on to the double and multiple stars—which now is found to be beautifully confirmed by the dynamical theory of the globular clusters. *It is not often that such a great law of nature can be brought to light, and it is worthy of the more consideration from the circumstance that it explains all classes of stellar systems by a single general principle.*

13. As sidereal systems of lower order are conserved by projectile forces, it is probable that the clusters likewise have a spiral motion of rotation, with similar projectile forces tending to counteract simple progressive collapse. The period of the orbital revolution of the stars of a cluster is found to be common to all, without regard to the dimensions of the elliptical orbits described; and thus the whole system may have a common period of oscillation, after which the initial condition is perfectly restored. This possibility in the dynamics of a cluster is exceedingly wonderful and results from the central attraction depending directly on the distance.

14. The equality of brightness in star clusters shows that some process of compensation between the attractive and repulsive forces

has produced stars of wonderful uniformity of luster. Thus the present investigation confirms the previous researches on the evolution of the stellar systems, which have laid the foundations for a New Science of the Starry Heavens.

15. Accordingly the Capture Theory of cosmical evolution being now firmly established for the clusters, where the nature of the process is entirely clear, it becomes at once a guide to us in dealing with systems of lower order; and we see that the law of nature is uniform and everywhere the same, the large bodies working in towards the centers of attraction, while the only throwing off that ever takes place is of small particles driven out of the stars by the action of repulsive forces. All planetary bodies are formed in the distance, and have their orbits reduced in size by increase of the central masses, and rounded up by moving in a resisting medium. *This is a perfectly general law of the sidereal universe. It verifies the early conjectures of Plato and Newton concerning the stability of the order of the world and shows that these illustrious philosophers were quite justified in concluding that the Deity always geometrizes.* The spiral nebulae tend to develop systems with rounder and rounder orbits, and the clusters made up of thousands of stars assume globular figures with minimal surfaces and internal density so arranged as to give maximum exhaustion of the potential energy.

16. This is geometry of the most marvelous kind, as we find it impressed on the systems of the sidereal universe; and the perfection of this most beautiful science of celestial geometry may be considered the ultimate object of the labors of the astronomer. The philosophic observer is not and never can be content with mere observations of details which do not disclose the living, all-pervading spirit of nature.

17. If, then, the mystery of the gathering of stars into clusters is now penetrated and traced to the clustering power of universal gravitation, so also is the mystery of the *converse problem of starless space,* which was a subject of such profound meditation by the great Herschel.

18. This incomparable astronomer likewise correctly concluded that the breaking up of the Milky Way into a clustering stream is an inevitable effect of the ravages of time; but we are now enabled to foresee the restorative process, under the repulsive forces of nature, by which new nebulae, clusters and sidereal systems of high order will eventually develop in the present depopulated regions of starless space.

UMI

19. If there be an incessant expulsion of dust from the stars to form the nebulae, with the condensation of the nebulae into stars and stellar systems, while the gathering of stars drawn together by a clustering power operating over millions of ages gives at length a globular mass of thousands of stars accumulating to a perfect blaze of starlight in the center, but surrounded externally by a desert of starless space resulting from the ravages of time, certainly the building of these magnificent sidereal systems may well engage the attention of the natural philosopher.

20. The foremost geometers of the 18th century, including Lagrange, Laplace and Poisson, were greatly occupied with the problem of the stability of the solar system; and in his historical eulogy on Laplace the penetrating Fourier justly remarks that the researches of geometers prove that the law of gravitation itself operates as a preservative power, and renders all disorder impossible, so that no object is more worthy of the meditation of philosophers than the problem of the stability of these great celestial phenomena.

But if the question of the stability of our single planetary system may so largely absorb the talents of the most illustrious geometers of the age of Herschel, how much more justly may the problem of the stability of clusters, involving many thousands of such systems, claim the attention of the modern geometer, who has witnessed the perfect unfolding of the grand phenomena first discovered by that unrivaled explorer of the heavens?

The grandeur of the study of the origin of the greatest of sidereal systems is worthy of the philosophic penetration of a Herschel! The solution of the dynamical problem presented surpasses the powers of the most titanic geometers, and would demand the inventive genius of a Newton or an Archimedes!

Yet notwithstanding the transcendant character of the problem, and the hopelessness of a rigorous solution in our time, even an imperfect outline of nature's laws may aid the thoughtful astronomer, in penetrating the underlying workings of the sidereal universe, and thus enable him to perceive the great end subserved by the development of the cosmos. If so, he may well rejoice, and exclaim with Ptolemy:

"Though but the being of a day,
When I the planet-paths survey,
My feet the dust despise;
Up to the throne of God I mount
And quaff from an immortal fount
The nectar of the skies."—Transl. by W. B. Smith.

T. J. J. See.

THE PROGRESS OF BUDDHIST RESEARCH;

WITH SOMETHING ABOUT PENTECOST.

Since the writing of my note on the "Buddhist-Christian Missing Link" in the fall of 1911 (*Open Court,* Chicago, and *Mahā Bodhi Journal,* Colombo, both for January, 1912), great events have happened in the field of Buddhist learning. We are now hot on the trail of the Missing Link, if we have not yet found it. For, besides the selected documents, to be presently described, there are thousands more reposing in the libraries of Pekin, London, Paris and Berlin, which we know to contain many more canonical Sūtras translated into Sogdian,[1] and there are doubtless more forthcoming in Bactrian also.[2]

In a book published in 1908, I said this:

"Menander, in the second century before Christ, showed an interest in, and a knowledge of, the Buddhist scriptures which may have been founded upon a knowledge of Pāli; but even then one would expect such a patron to have some specimens of the lore he admired translated into Greek, or into some vernacular. Strabo says that nearly the same language pervaded Media and parts of Persia, Bactria and Sogdiana. Strabo also says that the Corybantes had come from Bactria, and Euripides pictures them as passing the Bactrian Gates. When Buddhist ideas were carried westward, they would as surely be translated as the Bacchic had been."

These words were written not later than 1907, and since then my prediction has been abundantly verified. We have actually found fragments in Chinese Turkestan of Buddhist scriptures both in Bactrian and Sogdian, the latter coming from a Chinese library that was closed up in 1035, while documents from a near-by tower were dated A.D. 1 and A. D.20![3] Bactrian or Tokharish was the language of ancient Tukhāra, i. e., northern Afghanistan and parts of Chinese Turkestan. Sogdian was spoken in Russian Turkestan, where the city of Samarkand had been the center of a Greek civilization since the time of Alexander.

In Tokharish we have found Pācittiya 92 of the Vinaya, in the recension of the Sarvâstivādins, thus confirming the words of Yuan

[1] M. Aurel Stein, *Ruins of Desert Cathay.* London, 1912, Vol. II, p. 213.

[2] Sylvain Lévi, in *Le Temps,* Paris, May 19, 1911. Reprinted in the *Revue Archéologique.*

[3] Stein, *Ruins of Desert Cathay.* Among these documents, though undated, are some Sogdian epistles in Aramaic letters, now being read by Gauthiot.

Chwang, who said that all Tukhāra was Sarvâstivādin. In Sogdian we have found the Vessantara Jātaka, that great favorite about the Bodhisat prince who gave all he had away.[4] It was this very Jātaka that was graven upon the Great Tope at Anurādhapura, when visitors from Alexandria came to see the opening ceremonies, in the second century before Christ.

Other portions of scripture—the Nidāna and Dasabala Sūtras, the Dharmapada—and a patristic hymn, have been found in Sanskrit;[5] while fragments of patristics have also turned up in Eastern Turkish, written in characters of Syrian origin, side by side with a Christian legend about the Wise Men from the East in the Gospel of Matthew![6]

All this means that in the early centuries of the Christian era the religion of the Buddha was actively at work in languages spoken by the Medes and Parthians who were present at Jerusalem in the thirties of the first century (Acts ii. 9): "Parthians and Medes and Elamites, and the dwellers in Mesopotamia, in Judea and Cappadocia, in Pontus and Asia." It is to be noted that *Judea,* in this verse, is tautological, for the scene narrated is laid in Judea. As foreign countries are being represented, we must probably read *India,* though Dean Alford defends our present text on geographical grounds. Now the New Testament writer who tells us this is Luke, the Antioch physician, the author of a Gospel whose aim was to take Christianity outside the narrow pale of Judaism and put it into line with the Gentile religions. It is Luke alone who has the story of the Penitent Thief, corresponding to the Angulimālo of the Sūtras.[7] And in order to introduce this story into the Gospel, Luke is compelled to violate the text of his master Mark, who says that both the malefactors reviled the Lord. A scholar of the English church, in a recent number of the *Hibbert Journal,* has shown that Luke was utterly unscrupulous in literary matters, and again and again did violence to his sources to carry out his aims. I have suggested, both in my Tokyo edition (1905) and in my Philadelphia edition (1908) that Luke did violence to the text of Mark on purpose to introduce these Buddhist legends wherewith he was familiar.

It is true that our present Bactrian and Sogdian manuscripts

[4] Gauthiot, in the Paris *Journal Asiatique,* January-February, 1912.

[5] *Journal Asiatique,* Nov.,-Dec., 1910.

[6] *Abhandlungen* of the Royal Academy of Berlin, 1908 and 1911: article "Uigurica," by F. W. K. Müller.

[7] Middling Collection, No. 86, in the Pāli; but in the Numerical Collection in Chinese.

are probably to be dated between the third century and the eighth. But this is in Chinese Turkestan, whither their archetypes had been brought from regions to the westward. We know, from coins and from Buddhist history, that the religion was flourishing in Bactria both at and before the time of Christ; and the inference is irresistible that, when the missionaries began their Chinese translations in the sixties of the first century, they had already acquired experience as translators in the tongues of the Parthian empire. The only difference is, that the well-established civilization of China, and the continuance of Buddhism therein, have preserved and dated the Chinese versions, whereas the extinction of Buddhism by Islam in Afghanistan and elsewhere has destroyed those older ones.

What we have actually found of them is due to Chinese care, in Chinese dominions; but we are entitled to infer a whole lost literature in Bactrian, Sogdian AND GREEK,[8] which was the vehicle of Buddhist propaganda in the days of the Christian Evangelists.

We do not need to wait until a Greek Sūtra is dug up in Afghanistan, as I have hitherto anticipated. We now have actually in our hands a series of Buddhist documents translated by missionaries into languages that were understood by the very people whom Luke records as present at a feast which his authorities had witnessed. Could we but find, in these languages, the Buddhist Angelic Heralds and their Hymn, as recorded in the Sutta-Nipāto; the Lord's Three Temptations, viz., to transmute matter, to assume temporal power and to commit suicide, as recorded in the Classified and Long Collections; the Penitent Brigand aforesaid; and the Charge to the Sixty-one Missionaries, so like Luke's Charge to the Seventy, we should have in our hands the key to the riddle which Max Müller said he had spent his life in trying to solve; viz., the indebtedness of our proud religion of humility and peace, which has been spread over the planet by the swords of Europeans, to the meek and lowly cult of our brown brethren across the sea—that cult which, alone among the faiths of mankind, has never dipped its hands in the blood of animals or men.

ALBERT J. EDMUNDS.

PHILADELPHIA, PA.

[8] "There is little doubt that in Bactria, Buddhist literature was actually translated into Greek." (*Buddhist and Christian Gospels*, 4th ed., Philadelphia, 1908-1909. Vol. I (1908), p. 154). See also my remarks on "that lost version of the Sūtras which traveled westward." (*Buddhist Texts in John*, 1906, pp. 26-28.

BUDDHIST LOANS TO CHRISTIANITY.

WITH SPECIAL REFERENCE TO RICHARD GARBE.

SECOND ARTICLE.

To my remarks in *The Monist* and *The Open Court* for January, 1912, I should like to add a few words to congratulate Professor Garbe upon the conclusion of his learned monograph. His final summary I heartily endorse, except that I would modify one statement. The following is the paragraph referred to (*Monist*, April, 1912, p. 187):

"As we have seen, Christian influences upon the development of Buddhism are limited to secondary products of a late day; just as inversely Buddhist influences upon Christianity may be pointed out only in non-essential particulars and from times in which the doctrine of the Christian faith was established as a firm system.[ ] All identities and similarities in the teachings of these two great world-religions have, so far as *essential* matters are concerned, originated independently of one another, and therefore are of far greater significance for the science of religion than if they rested upon a loan."

These are essentially my own conclusions, stated many times since February, 1900; but I would add, at the brackets, the words: [*except a few passages of minor import which found their way from organized and aggressive Buddhism into formative Christianity.*]

The passages especially in my mind are the Angelic Heralds and their Hymn in Luke ii; the Lord's Three Temptations in Luke and Matthew; two texts in John expressly quoted as Law and Scripture, but not found in the Old Testament or any other Jewish book (John vii. 38; xii. 34); and the phrase *æon-lasting* (or "eternal") *sin* at Mark iii. 29—a phrase so foreign to Christian ideas that the copyists altered it to "eternal damnation," as Dean Alford admitted. Moreover, as said in *Buddhist and Christian Gospels* (Ed. 4, vol. 1, p. 157), Luke was probably influenced by such stories as the Charge to the Sixty-one Missionaries (his "Seventy") and the Penitent Brigand. As shown in my Tokyo edition (p. 48: the only important passage not repeated in the Philadelphia one) each of these stories of Luke is demonstrably fiction, and he moreover can be proved to have altered the Marcan or Synoptic tradition to suit his own ideas (as in Mark xvi. 7 = Luke xxiv. 6). To my mind the case is precisely analogous to that of the moons of Uranus being perturbed by the presence of Neptune.

When in Philadelphia last fall, Franz Cumont told us that there is a set of technical phrases in ancient Greek books on astrology which have now been shown to be literal translations from the Babylonian. In precisely the same way, such Buddhist phrases as *æon-lasting sin* and others gained similar currency among the ancients, *who persistently sought out the distinctive teachings of the great nations, just as we do now.*

With these reservations, I wish, as a student of Buddhism, to give my most cordial adhesion to the conclusions of the learned Brahmin scholar, who has dealt with a knotty problem in a masterly manner and summarized the researches of many specialists.

PHILADELPHIA, PA. ALBERT J. EDMUNDS.

A POSTSCRIPT TO INDO-ROMAN RELATIONS IN THE FIRST CENTURY.

In *The Monist* for December, 1911, Professor Garbe denied the existence of Buddhist loans to canonical Christianity, and gave as one of his reasons the following question and answer:

> "Do the evidences of intercommunication at all permit the *assumption*[1] that as early as the first century after Christ, or earlier, Buddhist legends and ideas had found their way into Palestine?....
>
> "They are not apt to raise this possibility to a serviceable degree of probability for as early a period as the first post-Christian century."

To this assertion I replied in the following number, avoiding reference to the canonical literature, which was simultaneously considered by my friend Mr. Albert J. Edmunds, but assembling various evidences of a large, important and rapidly increasing intercommunication between Rome and India during the first century of the Christian era, as indicating the possibility of the assumption which Professor Garbe had outlined.

In *The Monist* for July, 1912, in a postscript to his most instructive discussion, Professor Garbe acknowledges the probability of closer intercommunication than he has heretofore admitted, and accepts one of the canonical parallels offered by Mr. Edmunds; but he thinks that I "beg the question" by *assuming* the possibility of an interchange of ideas as well as goods.

To this objection I would reply that I was but addressing my-

[1] Italics mine.

self to the *assumption* which he had declared to be unwarranted; so that if there has been any begging of the question it would seem to have been in the same degree on each side of the discussion.

Professor Garbe objects to any citation of the Periplus because it does not mention religion; but the citation was merely to show the existence of an active commerce, and it is well known that the missionary and the trader have gone close together in many ages of the world. They have not always respected one another, but they have usually followed the same paths. Surely Professor Garbe would not expect a future historian of our own times to deny the *assumption* of Christian missions in China because some surviving consular report on the Shanghai trade might omit a reference to the Nicene Creed!

Professor Garbe objects, also, that the Hindu traders to the Roman Empire were Dravidians and stupid, and therefore not likely to talk of their religion. But in the first century of our era they were increasingly Indo-Scythian, from a portion of India that professed a liberal and proselytizing Buddhism, and I repeat that for that date and race, a spreading of ideas together with an interchange of goods, was not only a possible *assumption,* but a probable fact.

The extent of such intercommunication is made much more evident by Mr. J. Kennedy's paper "The Secret of Kanishka," begun in the *Journal of the Royal Asiatic Society* for July 1912. The long-drawn discussion as to the so-called Vikrama era of 58 B. C. seems brought to a reasonable conclusion through Mr. Kennedy's brilliant assembling of Chinese and numismatic evidence. It was the era of the second Buddhist Council and of the Kushan king Kanishka. His power over northwestern India, built up by his control of the transcontinental silk-trade, was fortified by his becoming the protector of the Buddhist faith; and under him and his immediate successors, just before the Christian era, it is highly probable that his faith was expounded to the east as far as Turfan, and to the west as far as Charax Spasini, Antioch and Alexandria.

The truth is, that during the period between 50 B. C. and 100 A. D., approximately, India was a leading factor in the world's thought, industry, commerce, and wealth; and, this being the case, to repeat Professor Garbe's own words, "the evidences of intercommunication permit the assumption of the migration of Buddhist legends and ideas into Palestine as early as the first century after Christ."

WILFRED H. SCHOFF.

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BOOK REVIEWS AND NOTES.

THE RELIGION OF THE IRANIAN PEOPLES. By *C. P. Tiele.* Part I (From the German). With Darmesteter's Sketch of "Persia," and Goldziher's "Influence of Parsism on Islam" (From the French). Translated by *G. K. Nariman.* Bombay: Parsi Publishing Co., 1912.

G. K. Nariman, who is known to readers of the *Revue de l'Histoire des Religions* and other magazines, is a Parsi scholar who is also acquainted at first-hand with Buddhism, both from its Pāli and its Sanskrit sides. Indeed, one of the features of this excellent translation is an appendix by the translator entitled, "Some Buddhistic Parallels." The subjects dealt with are: (1) the triple formula: body, speech and mind; (2) Buddhist allusions to consanguineous marriages; (3) Hindu exposure of the dead, as described in the Pitakas; (4) Māra and Ahriman; (5) questions addressed to Mazda and Buddha; (6) the idea of a counterfeit creation (Parsi) or a counterfeit religion (Buddhist); (7) the use of the name Ormazd by Turanian Buddhists; (8) the killing of noxious creatures by the Kambojas; and some other points.

The following note on the Turkish Buddhist literature recently found in Central Asia deserves reprinting:

"*Ein in turkischer Sprache bearbeitetes buddhistisches Sūtra,* by Radloff and Stael-Holstein (St. Petersburg, 1910). This important work is typical of the avoidable Babel which Western philologists seem unfortunately determined to create in their otherwise fascinating field of marvelous investigations. The original text is in the, up to now, almost unknown Uigurian, which the Russian scholars have made accessible to us through a German translation; but the transliteration is in Russian character, and the interesting notes on the Brahmi gloss are made unserviceable to the average student of Buddhism by the introduction of two sets of unknown alphabets, besides Chinese, Arabic and Nāgarī scripts. Eastern students, however, must be grateful to the Imperial Academy of Sciences, St. Petersburg, for the employment of the Nāgarī character in the publication of its admirable series of *Bibliotheca Buddhica.*"

It is studies like this which help to break down the former provincialism of religious treatises, wherein (to borrow a phrase of Cumont's) each religion was regarded as an experiment in a closed vessel.

We could wish that the learned translator had had a little more regard to the naturalization of Oriental adjectives, and so given us the familiar English forms *Achæmenian, Sassanian,* instead of *Achæmenide, Sassanide.* The lack of diacritical marks is also confusing. But such small flaws by no

means detract from the great value of Tiele's able essay in an English dress, accompanied by the other essays indicated, which make the little book an interesting companion for the student of Comparative Religion.

ALBERT J. EDMUNDS.

ALTUTONISH (PANGERMAN). BEI ELIAS MOLEE. Tacoma, 1911. Pages 32.

The advocate of a new language called "altutonish" or "pangerman" together with an abolition of all capital letters is vigorously continued by "elias molee, ph. b., 1554 'd' street, tacoma, wash., u, s. a." by sending out a pamphlet containing exercises in his new language which he characterizes as " ein (a) union spiek (language), makn up ov deuch, english, skandinavish and hollandi, for to agenfererein (re-unite) al tutonish folka (people) into ein spiek mitin (within) feivti (50) jiera (years)."

He believes that the world is mainly Teutonic and that a combination of all Teutonic languages should be the best international language. As a motto he selects a word of Victor Hugo, who has said somewhere: "The German character hovers over the nations," or as it reads in German: *"Die deutsche Natur schwebt über den Völkern."*

Such a combination might have been possible at the time the English language originated after the Norman conquest through the breakdown of the literal Anglo Saxon; but times are changed through the wide-spread use of written and printed language which has added power to the resistance of the established language such as was impossible in the times when language was still purely speech, when it was limited to the spoken word which is more flexible and would admit easily of radical changes. K

Readers of *The Monist* will be interested to hear that Prof. Hugo de Vries is making another visit to the United States this autumn. He reached New York about September 12, where he was to give a lecture at the Botanical Garden on September 14. From there he goes to the University of Pennsylvania to see the work which Professor Davis is doing with *Oenotheras,* and then spends a short time in Washington. His next objective point is Dixie Landing, Ala., where he goes with Professor Tracy to visit the type locality of *Oenothera grandiflora* to study its possible mutants in its original habitat. He then goes to Biloxi, Miss., where he will make his headquarters while visiting the "mud lumps" near the mouth of the Mississippi River, and a number of islands near Biloxi. After that he and Professor Tracy will go to San Antonio, Brownsville, and other points in southern Texas, where Professor de Vries goes to study the relation of the flora to the geological and geographical conditions. On October 14, Professor de Vries is to deliver the dedicatory address of the Rice Institute, at Houston, Texas.